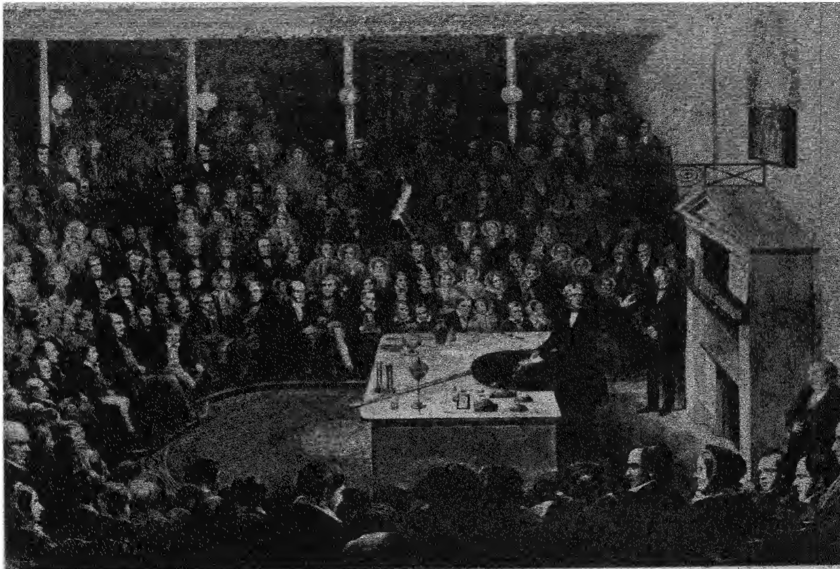
FARADAY'S DIARY

VOL. VI



FARADAY LECTURING AT THE ROYAL INSTITUTION, H.R.H. THE PRINCE CONSORT IN THE CHAIR. CHRISTMAS JUVENILE LECTURES, 1855–6

From a coloured lithograph (made by the artist) after the oil painting by Alexander Blaikley, now in the Hunterian Museum of the University of Glasgow

FARADAY'S DIARY

Being the Various Philosophical Notes of Experimental Investigation

made by

MICHAEL FARADAY

D.C.L., F.R.S.

during the years 1820–1862
and bequeathed by him to the

ROYAL INSTITUTION OF GREAT BRITAIN

Now, by order of the Managers,
printed and published for the first time,
under the editorial supervision of

THOMAS MARTIN, M.Sc.

with a Foreword by

SIR WILLIAM H. BRAGG, O.M., K.B.E., F.R.S.
Director of the Laboratory of the
Royal Institution

VOL. VI

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PREFACE TO VOLUME VI

FOLIO Vol. VI of the manuscript contains a considerable number of the actual specimens prepared by Faraday in 1851 to illustrate the delineation of lines of magnetic force by iron filings. The filings were fixed on cartridge paper by means of gum water, sometimes mixed with a solution of "the red ferro prussiate of potassa" (apparently potassium *ferri*cyanide), which was found to leave a blue impression of the pattern after the filings had been removed or worn off. The method of preparation is referred to on pages 9–10 of this volume and also described in par. 3236 of the Twenty-ninth Series of Experimental Researches in Electricity (*Exptl. Res. Electy.*, vol. III, p. 398).

Some of these specimens are still in a very good state of preservation; others have deteriorated, particularly some of those in which the ferricyanide was used: the filings have worn off and the blue colour has spread all over the paper, making the pattern indecipherable. In a number of cases the red of the oxidised iron particles is left against a blue background, and fortunately this combination of colours reproduces well photographically. A selection has been made, and those specimens considered suitable will be found reproduced (by the collotype process) in Plates I to VII.

Folio Vol. VII of the manuscript contains three large folding sheets of graphs, in which Faraday plotted his measurements, made in the summer of 1855, of the variation of magnecrystallic force with temperature for several different substances. The numerical data on which these curves are based will be found tabulated in the Diary (see notes in square brackets on pages 424, 427 and 491). The curves themselves are so large and so finely

drawn and lettered that they could not be conveniently reproduced in their original form. They have therefore been omitted.

The frontispiece to this volume is of especial interest. It represents Faraday giving the Christmas Juvenile Lectures of 1855–6, on The Distinctive Properties of the Common Metals. The course was attended by H.R.H. The Prince Consort, accompanied by his two sons, the Prince of Wales (afterwards King Edward VII), who is seated on his right, and Prince Alfred (afterwards Duke of Edinburgh), on his left. Sir Charles Lyell, Sir James South, Dr Warren de la Rue, Dr Bence Jones, Professor Tyndall and others well known at the Royal Institution at the time appear in the audience. Faraday's assistant Anderson stands behind him with hands crossed. The original oil painting, by Mr Alexander Blaikley, was done from sketches obtained at the lectures. Lithographic copies of the painting were afterwards made, from a drawing on the stone prepared by the artist himself. It is from one of these lithographs, hand tinted, which is in the possession of the Royal Institution, that the present reproduction has been made.

T. M.

ROYAL INSTITUTION
February 1935

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Continuing the practice in the previous volumes, the cross references to Faraday's published work included in the Contents are, as far as possible, to the collected editions of his papers. The majority of the references in the early part of this table are, it will be seen, to the *Experimental Researches in Electricity*; but the third and final volume of that work appeared early in 1855, and electrical papers published after that date must be consulted in the journals in which they were originally printed. Thus the Thirtieth Series of Experimental Researches in Electricity (the last of that sequence) and certain other papers referred to below, were not included in the collected edition, but will be found in the *Philosophical Transactions of the Royal Society*, the *Proceedings of the Royal Institution* and other journals to which the references have been given.

FOLIO VOLUME VI OF MANUSCRIPT (*continued*)

1853

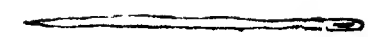
1854

PLATES

FOLIO VOLUME VI OF MANUSCRIPT
(CONTINUED)

11666. Wanted to know how the lines of force were disposed in and about magnets and Iron generally under certain circumstances of position, and for this purpose used *fine* iron filings upon paper over the magnets, sprinkling them evenly and tapping the paper lightly.

11667*. First a simple magnet, being a large needle of about this[1] size, well magnetized by a horseshoe magnet of power. It gave beautiful curves having perfectly simplicity of form, but is to be remarked that N or S lines issued not from the ends of the needle but far down towards the middle. In order to distinguish the ends of the lines, we may call those at N, N issues or ends or nodes; and the middle part *c* the equatorial center or ventrum.

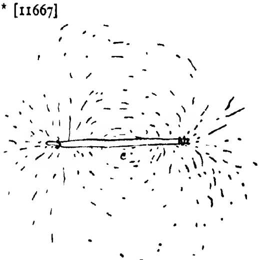


11668. When the needle was broken into two parts, each part by itself acted well as above (11667). When it was put together again, it was no more as one magnet but thus†; there being four consecutive poles and consequently *two* equatorial ventrums, or rather three, but the middle one at the junction very short and compressed and the direction of the curves outside of the magnet the contrary of that of the parts outside to the right and left.

11669. Now indeed it appears that certain of those curves which before were entirely within the body of the magnet are expelled into the air, because of the sudden diminution of conducting condition at that spot by rupture and want of continuity, and that of those which thus come out through the sides of the magnet, part returns and is discharged at the nearest pole and part goes on and dips into the further half. So that the bundle of lines of force are divided generally thus‡ into three parts—a part which goes down the middle of the magnet, right across from one to the

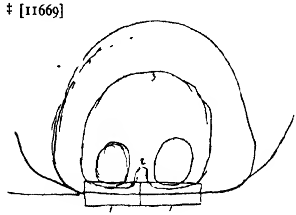
[1] Reduced to $\frac{3}{4}$ scale.

* [11667]



† [11668]

‡ [11669]



other at the break, out at the poles, the circuit being completed in space above as in the unbroken magnet; a second part which issues out at the break, but goes on and dips in again and goes out towards the poles on each side or end, to be completed in the space above but within the region of the former lines; and a third part which, issuing at the break, forms (by returning) two systems. These are the external lines of force which belong to each half needle acting as an independant magnet. The two others are the external (chief) lines of the two halves of the needle acting as a whole magnet. But the first part or portion is the only one whose lines are confined within the dimension of the magnet in the whole of its course.

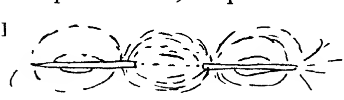
11670. All the lines of force within the magnet are only so at the equatorial centers or ventrums 1, 1; as in a perfect magnet they are within it only at the equatorial part. Of the other parts, some of those which go into the space around at the pole are also in the space around at the junction, where of course they have an opposite direction. Consequently somewhere between 2 and 3 there must be a neutral place or a place where no magnetic force is exerted, at all events as regards direction (11672, 7).

11671. When the two halves of the needle were opened out thus*, the middle air portion of the curves was well developed. But though the filing lines were more developed there, it is certain that less power passed across or on them than before, and less and less as the space was increased. At the same time, more returned back from the inner end of each half to the outer end, i.e. the third portions (11669) increased continually. And they would do so until at last they would become the independant systems of the two completely separated halves, which then would exist as two magnet[s], each having its own equator or maximum place of inner curves which would contain just as much force as the original equator, provided the magnet could be broken without a letting down of the state within.

11672. Then the neutral place (11670, 7) would be at an infinite distance.

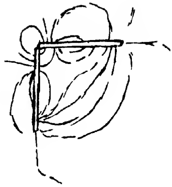
11673. The two halves of the needle were carried through a series of successive positions, so as to make them represent the horse shoe and compound cases; the positions were these† in the

* [11671]

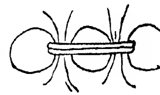


† [11673]

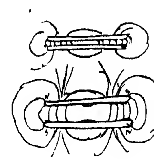
first instances. Now when in a line, with unlike poles together so as to make one long magnet—the curves have been noted. When placed at right angles, of course unlike poles together, then the curves were generally as represented. It is easy to see how this disposition arises and is developed from the former ( ) and how some of the curves of the third portion are now removed into the second, going across the air both at the poles in contact and at those which are now 90° instead of 180° apart.



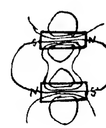
11674. The two halves were now brought together, as if one had made a further movement of 90°, so that they laid side by side with like poles together. In this case the course of the lines of force between the magnets were lost sight of and only those depicted remained for the filings to shew. It is easy to see how they arise from the former disposition, but the system is now weak and it must be remembered that by far the greater number of lines of force are passing directly across from magnet to magnet and are not sensible here without close inspection.



11675. To make these visible the little magnets were opened out slightly and then the lines were thus—in which it may be seen how, as the magnets are placed wider apart, the equatorial ventrum of each small magnet begins to appear as the lines of force of each magnet tend to go back from one pole to the other, rather than across the intervening space to the opposite pole of the opposed magnet. When the space was further increased, then the distribution of the lines was thus*; and when it was still further increased, it was as in the left hand figures†. All these cases are easily referable one to the other and flow as a very natural consequence from the nature and character of the lines of force.

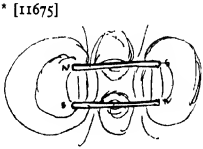


11676. In the last case for instance, the lines through the magnet and outside generally as represented. Some pass through both magnets, while some turn short round and do not; and the maximum internal effect is resident within the equator of either magnet.

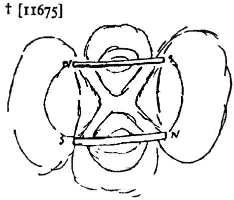


11677. The neutral region (11670, 2) is here very distinct and it is easy to see that a particle of iron placed there will *not be attracted* or *point*. Must trace this to the neutral place in (11670). That

* [11675]



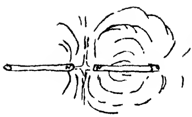
† [11675]



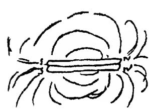
can hardly be at an infinite distance or even at a distance where the forces are insensible.

11678. When two bar magnets each a foot in length were placed in like positions to those described, with contrary pole[s] together or in relation to each other, they gave the same results.

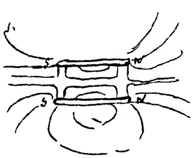
11679*. The two small needle magnets (   ) were now placed in a line with like poles near each other. The systems of curves or lines of force had the expected and well known disposition, being compressed at the place of contact and limited each for itself by a plane there at right angles to the line of the magnets. When opened out a little, the disposition was in principle the same, and it is easy to see how by great removal apart the two magnets acquire their fully developed systems in their ordinary form.



11680†. When these were placed at right angles, then the lines within the angle were compressed together and those from the outer or S poles began to be thrown out; and when the moving part was carried through 90° more, so as to lay the magnets side by side, the inner lines of force disappeared between the magnets by a very natural transition, and the two magnets acted as one. A section through both would of course give twice the number of lines of force as a section through one, and if each magnet were to retain its full power under all these alterations, such a disposition would give double external power as indicated by lines of force than if the two magnet[s] had been placed end to end. Must consider this in the case of magnetization by helices, etc. etc.



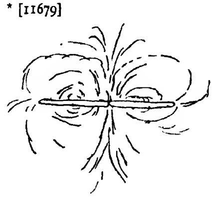
11681. When the two magnets, being together and parallel, were opened a little thus, then the forms of the lines were as shewn, the ærial expansions at the equators appearing between; and it is evident how by further removal the two systems would gradually resume their perfect form.



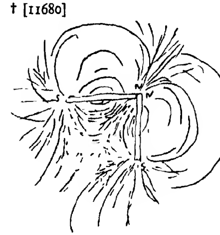
11682. It is exceedingly beautiful to see in all these arrangements how beautifully the lines of force represent the disposition of the magnetic power.

11683. Considering Air, Iron and all things external to the magnet as mere conductors of the magnetic lines of force, I observed the

* [11679]



† [11680]



curves by filings over one of the small needle magnets when sustained on *wood* and also again when lying on a mass of soft unmagnetic *iron.* The form of the curves as shewn by the filings was just as good in the one case as the other, but the quantity of power or force which gave the form was very much diminished in the latter case; and though it was manifest by the disappearance of much power from the air that the iron was a far better conductor than it, still a good deal of power remained to be transferred through the air (11960, 1, 2, 3, 4).

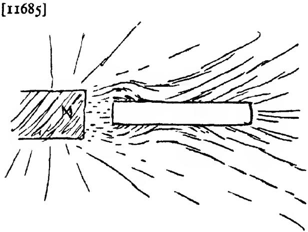
11684. As the power is definite and does not change in its amount though it does in its disposition in the two, it seems to me impossible to make a distinction between it in the iron and in the air, except as to amount. The polarity of the lines of force is the same in both cases and the iron has no more power of retaining them than the air, or water, or wood or other matter.

11685*. Again, placed a piece of soft iron opposite the pole of a bar magnet a foot long, an inch broad and 0·4 thick (the figure is to a scale of about one half[1]); and then placing paper over the bar and magnet, sprinkled fine filings on and observed the lines depicted. It was beautiful to see how they flowed into the iron at the end near the magnet and how they flowed out again at the further part from a comparatively much larger surface—and also to see the concavity of the lines outside the iron near the equatorial part of it, shewing the double curvature—and the beautiful character of the streams of force into the air or space from the further part.

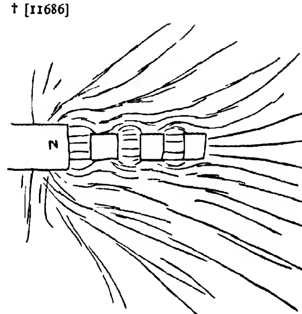
11686†. Again. Several short thick pieces of soft iron, being parallelopipeds, were adjusted before a magnet, air intervening, and here the entry and exit of the lines of force at the different cubes or pieces was very beautiful, giving a fine display of the

[1] Further reduced to $\frac{3}{4}$.

* [11685]



† [11686]



disposition of the power. The filings tended to leave the faces over the iron and go to the parts over the air intervals, shewing the greater manifestation of power or stronger intensity of the lines were passing through worse conductors than where they had dipped into the iron and were there carried on. The side undulations too shewed this disappearance and reappearance of the lines of force at the sides of the iron, and this was shewn again by raising the paper supporting the filings *from the iron* faces. For whereas the filings, when close to the iron, did not depict lines of force but just went as the agitation favoured their movement to the parts over the air, i.e. from weaker to stronger places of action; when the paper was raised, then the forms of the curves reappeared over the iron, first at the edges and, as the paper was more raised, at the middle part, and with beautiful delicate indicating curvatures respecting the equatorial part of each piece of soft iron.

11687. The manner in which the iron robs the space around its equatorial part is very striking, and I have no doubt that a little needle *close* to the iron at either the top or bottom or side faces as regards the axial direction, would be indifferent to the lines of force flowing all about it (11960, 1, 2, 3, 4).

11688. So an ordinary magnetic needle placed on a large block of iron is altered in its relation to the earth, and I have no doubt that a dipping needle having a block of iron placed parallel to it—or being inside a thick tube of iron—would be taken out of the earth's action considerably and indifferent to it.

12 NOVR. 1851.

11689. A piece of *hard steel* in the place of the long soft iron (11685) acted very slightly in the same way that it did—but not to be compared in amount of effect. It also attracts either end of a magnetic needle as iron does. One would like to obtain a piece of steel so hard that it should refuse induction and be as air, if not brought in contact. The case occurs almost with a bar of hard steel, a very small magnetic needle and the earth's lines of force.

11690. Worked with the 12 inches bar magnets and the six inch bar magnets for the curves on paper by filings and for the neutral

point (11670, 2, 7). The 12 inch bars are too long. The six inch bars will do well for length, but require magnetizing by the electromagnet. At present they are very irregular, and the filings shew it well when the two are opposed thus. The place of the neutral point is very well obtained with them (   ).

11691. *Filings.* The finer they are the better for the production of neat lines by little use of extraneous matter.

11692. Floating on water will not do—they cling together.

11693. The lines obtained on a sheet of glass over the magnets are very good with large magnets, but hardly do with small needle magnets because of the thickness of the glass. Else the curves are very regular, because the glass does not cockle.

11694. Silvered or Gilt paper does well, the filing[s] slipping into place very easily. Useful in the investigation of neutral places, etc. where the power is small.

11695. Now proceeded to fix the curves as depicted by the fine iron filings, and succeeded in several ways. Thus, the half needle magnet was placed between the edges of two cards so as to make a flat surface, then a piece of cartridge paper, flat, laid over it; sprinkled over with fine iron filings, and the paper tapped with a splinter of wood here and there until the filings were well arranged. Then a similar piece of cartridge paper was brushed over with moderately thick gum water and a camel hair brush—laying the fluid evenly—and wafted through the air a few times, which breaks the air bubbles produced by the brushing. After this it was carefully laid on to the paper sustaining the filings—a cushion of 16 or more folds of filtering paper placed over it—a thick flat plate of glass on that, and then pressure given by the hand or a 56 lb. weight for half a minute or more. On being taken up, all the filings in their proper places were attached to the gummed paper and when that was dried were fast attached to it. No. 1 on the next page [Plate I] is the very specimen and the first made.

11696. Then tried to print off the curves, and therefore in place of gumming the second paper, it was washed over with a solution of yellow ferro prussiate of potassa and pressed on as before. It took up the filings and they were allowed to remain on until dry; but being then brushed off, the traces left behind were scarcely visible and I have destroyed the result as useless.

11697. On brushing over the second paper with a moderately dilute solution of the red ferro prussiate of potassa, and applying it in the same way, the final result was a very excellent delineation in Prussian blue of the position of the filings and the curves. The results will do perfectly well as drawings to go with the paper and to the artist. Nos. 2[1] and 3[1] are the two first specimens so produced.

11698. No. 4[1] is a similar preparation but the filings were sprinkled on a plate of glass laid over the magnet, instead of being sprinkled on paper.

11699. Also employed gum water and the red ferro prussiate mixed, so as to retain the design in filings as long as it would wear and leave a design in Prussian blue afterwards. No. 5 is an illustration [Plate I], and shews how finely the lines may be traced with fine filings.

13 NOVR. 1851.

11700. Rubbed the filings in a mortar and sifted out the fine particles. But it being principally oxide dust, though it forms curves, they are not so beautiful as those with clean small filings – nor so well taken up by the gummed paper.

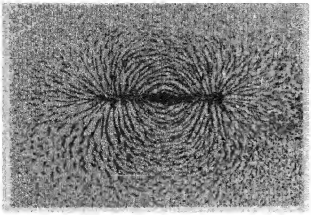
11701. With large displays, the size of this leaf[2] for instance, it is best to lift the paper carefully up from the magnet, to lay it on a flat surface, and when the Gummed or test paper is put over it, to put on 6 or 8 thicknesses of filtering paper and, holding all tight, to rub it down on the filings. With care they may be well taken up this way. But pressure is better for small printings or designs, only the sustaining surface should be flat and equal.

11702. When a thin plate of steel, very hard, is magnetized and then broken down into short length[s], the short wide magnets are very interesting. For instance, a short one such as figured will give curves extending to twice or thrice its own length all the way round.

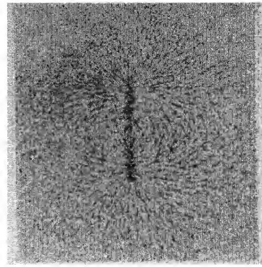
11703. Had a long steel wire 15 inches long and not above 0·05 of an inch in diameter. It is difficult to magnetize it, though soft, as one magnet, consecutive poles starting into existence; but did so at last by our helix (large one). Then took its picture as to

[1] Not reproduced.
[2] Foolscap size.

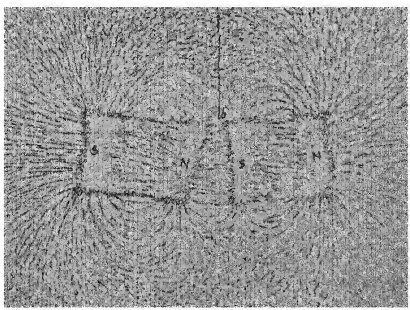
PLATE I



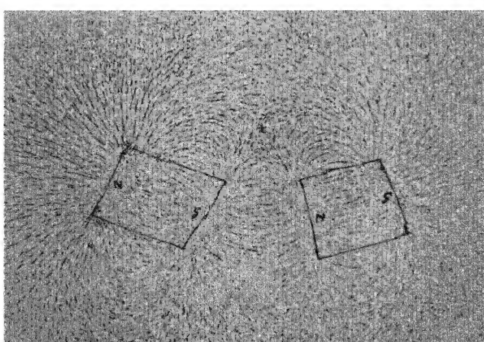
Par. 11695. No. 1. Gum water alone (*three quarter scale*)



Par. 11699. No. 5. Red ferro-prussiate of potassa and Gum water (*three quarter scale*)



Par. 11704. (Upper figure, *two thirds scale*)



(Lower figure, *two thirds scale*)

curves. Broke it in half and did so again, and I think the two halves put together with like poles in the same direction was stronger than the one whole length. Broke each piece in half again and put the four with like poles together in one bundle. This short and thicker magnet was stronger than the one long one as to throwing out curves.

15 NOVR. 1851.

11704. Ascertained with two short thick magnets the curves and the neutral place [Plate I, two figures], with a view to test the directions of motion of a piece of soft iron when placed at the neutral place and on different sides of it. Selected the case given in the upper figure, and having fixed the two magnets in position on a card, arranged a delicate torsion balance and at the end of the lever a small piece of soft unmagnetized iron about this size (–), so that it should lie in the plane of the magnets when these were brought near. Now when the balance and the magnets were so arranged that the soft iron was in the position – at the neutral place, and therefore as a tangent or chord to the curves crossing the line *a b*, being also restricted so that it could move to and fro only along the line *a b*, then when between the neutral place + and *b* it was attracted by the magnets, and more and more powerfully as it was nearer to them; but I could not make out that when outside +, it was repelled, as was expected, where the curves make a double dip there. At times it seemed to be so, but the power if any was so very small that the currents of air in the covered box of the torsion balance were still strong enough to entirely hide them, and yet they were but weak. So there can be only very little power, if any.

11705. Upon consideration, I think that the action of four points assumed in the ends of the two magnets would give the same results as the lines of magnetic force, both modes of representation agreeing here (   ).

17 NOVR. 1851.

11706. Have received from Newman my new square thick wire Galvanometer; have magnetised the needles and put them into place. It answers exceedingly well and is much more powerful for abundant low currents than the former. It also does much better for chemical action than I expected, for pieces of Zinc and platina at the mouth deflect it 45° or more. The coil wire is square, 0·2 of inch thick—copper—and forms two convolutions round each needle, the needles hanging between the two, so that it has twice the convolution of the former Galvanometer (11490, 525).

11707. The copper is a little magnetic, so that the needle cannot stand at zero or equidistant between the two coils. I believe this is due to some superficial iron or dirt, and must go over the surface with sand paper.

11708. The different contacts are now the difficulty. Those at the galvanometer I make by cups of mercury. Those of the rectangles with the commutator plates I think I shall make by soldering. Those between the rubbing parts of the commutator are the most difficult to adjust so as to make them sufficient, constant and free.

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11709. Working with the New Galvanometer (11706), loops and the bar magnets, in respect of the amount of lines of force associated with each magnet individually, and also the manner in which they are affected when the magnets are associated with each other in various ways. The Galvanometer was placed on the table and its needle freed from local action in part by a small bar magnet placed in a right position. It then made one swing either way in 20 beats of my watch.

11710. The loop used was of the kind before described (11552), with thick wire for three feet nearly; the ends of this loop dip into two cups of mercury, into which also dip the ends of the Galvanometer coil.

11711. The Magnets used were the two twelve inch bars of former experiments (11322) now marked A and B. One of these was

set up on end so that the loop could pass over the pole to the equator, and a block prevented it from proceeding farther. Thus the loop took all the curves or lines of force of the magnet once (i.e. very nearly all and allways the same proportion). When this magnet was in place, it affected the Galvanometer needle, being in fact too near to it. A foot bar magnet may well do so. It tended, when with the N end upwds., [to make] the time and the extent of the swing of the needles longer. Also as it was placed East or West of the line joining it and the magnet, it tended to set the Zero of the indicating end East or West of the mean zero. When known, this could be allowed for, but it will be better to work farther off. All loose surrounding magnets ought to be cleared away and iron and magnetic things left stationary during experiments.

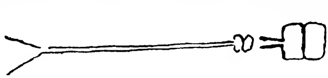
11712*. The loop employed to-day was of this form. The wire of the double loop was 0·06 of an inch thick and 13 inches in length. The loops or apertures were each about $2 \times 1\frac{1}{4}$ inches, so that they could pass over two poles or bars at once, or over one only, the same length of wire remaining in the conductor in both cases. Of course the wire did not touch at the crossing, but was insulated every where.

11713. The bar magnet A being set up on end and all ready, the loop was passed over and the deflection swing observed. When the needle was at Zero or quiescent, the loop was passed off and the swing again observed; and the results are set down below.

Bar A. On	50°	50°	51°	50°	57°	55°	55°	average	52°·57	mean 41°·71
Off	29°	29°	30°	32°	32°	32°	32°	„	30°·85	

11714. In this first experiment, it is to be understood that the side magnet was so near as to affect me [? the] magnetic needles much more when they were deflected in one direction than when in the other; and hence it is that the passing of the loop over the pole and the passing of it off or away, seem to have different degrees of strength in producing an electric current. Electric currents of contrary directions and exactly equal in strength are really produced, and this is shewn by quickly passing the loop over and off again, when the needles are left stationary and counterbalanced in action.

11715. The difference after the fourth observations was due to

* [11712] 

the removal of some distant side magnets. All should be clearly arranged before hand and then left undisturbed.

11716. Now the bar B was employed with the N end upwards (11713).

On	33°	29°	31°	29°	29°	33°	Average 30·66	mean $24^\circ{\cdot}41$
Off	20°	20°	20°	20°	19°	20°	„ 18·16 [*sic*]	

11717. Bar B with S end upwards.

On	18°	19°	Average $18^\circ{\cdot}5$	mean 20°
Off	21°	22°	„ $21^\circ{\cdot}5$	

11718. Bar A with S end upwards.

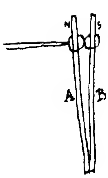
On	26°	26°	average 26°	mean 28°
Off	31°	29°	„ 30°	

11719. Bar A with N end up.

On	50°	49°	average 49·5	mean $38^\circ{\cdot}25$
Off	27°	27°	„ 27	

11720. In the foregoing experiments, it is not to be supposed that the bars differ thus much from themselves as the N or the S end is uppermost. The fact is that the needles are affected by the bars when in these positions, and that with the N end upwds. they are more astatic and so more delicate and swing farther than when the S end is upwds. The bars and the Galvanometer must be separated to a greater extent (11733, 8).

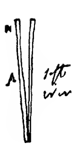
11721. The two bars appear to differ in strength, and that I believe to be a real difference.



11722. Now arranged to work with two poles or extremities at once or with one only as before. Two bars might be set up so that the loops might pass over both at once, or the outer or further bend over one only. Thus the two bar magnets A and B were put up, touching by contrary poles below but open above, and then the double loop was passed over the poles to the equator as in the figure. The results were:

On	83°	84°	84°	average $83^\circ{\cdot}66$	mean $80^\circ{\cdot}83$
Off	76°	79°	79°	„ $78^\circ{\cdot}00$	

Shewing generally the power of two poles or the use of both ends of the magnet, over one pole or half the magnet.

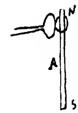


11723. Adjusted bar A and a soft iron bar of equal size together, and now the results were:

On	68	69	71	average $69^\circ{\cdot}3$	mean $67^\circ{\cdot}15$
Off	65	65	65	„ 65·0	

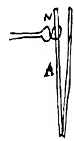
So that a bar of soft iron does not equal the bar B when used in its place.

11724. Wished to see how the lines of force about one pole or end of a magnet were affected in strength or quantity by association with other magnets or bars of iron. So employed the outer loop to pass over the pole and again used the Bar A with the N end upwards, thus:



On	30	27	25	30	30	average $28^\circ{\cdot}4$	mean $35^\circ{\cdot}9$
Off	44	44	46	42	41	„ $43^\circ{\cdot}4$	

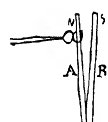
11725. Associated the bar A with the 12 inch bar of soft iron thus, so as to cause great disturbance in the arrangement of the external power, and pass very much of it through the soft iron bar which before went through the air. Then



On	35	35	36	average 35·53	mean $41^\circ{\cdot}43$
Off	47	47	48	„ 47·33	

So the bar A throws out much more external power now than before. Probably because external system improved and internal condition of restraint partially removed.

11726. Associated bar A with the bar B, opposite poles being in relation, thus. Then



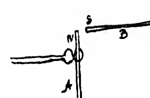
On	40	38	38	average 38·66	mean $43^\circ{\cdot}5$
Off	49	48	48	„ 48·33	

So that bar B is even better than the bar of soft iron in developing the outer (and inner) system of bar A.

11727. Still it is abundantly manifest that the power of B is not added on to the lines of force between it and A; for with A alone or in air, it is 35·9 or 36° nearly, and with A + B it is only $43^\circ{\cdot}5$.

11728. If A and B were *very hard*, *thin* magnets—there would be probably much less difference than this.

11729. Now arranged the collateral bar B thus, that its power might be more simply added on to that of A.



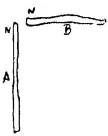
On	31°	31°	32°	33°	34°	33°	Average 32·33	mean 34°
Off	34	34	37	35	37	37	„ 35·66	

So is even less than with A alone (11724), but this I believe is because when B is there, the power of the N end of A is taken off from the galvanometer needles.

11730. Took away bar B, so as to restore A to its former isolated condition (11724).

On	33	35	33	average 33·66	mean $37^{\circ}{\cdot}66$
Off	39	42	44	„ 41·66	

The power of A appears to have been raised by association with the bar B, and the result is a very natural one and well brought out by this apparatus, even in these first rough experiments—now it is $37^{\circ}{\cdot}66$, before it was $35^{\circ}{\cdot}9$.

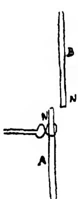


11731. Replaced bar B, but with the like pole near the N pole of A, which of course as respects action on the galvanometer would much increase the power exerted by the single bar A and so make its indications more large. The results were

On	37°	38°	35°	36°	average $36^{\circ}{\cdot}5$	mean $41^{\circ}{\cdot}25$ (11734)
Off	47°	44°	47°	46°	„ 46°	

11732. So when pole N of bar A was alone in use the effect was $37^{\circ}{\cdot}66$. When associated with the contrary pole of bar B, the effect was 34·00. When associated with same pole of bar B, the effect was 41·2.

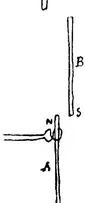
11733. No doubt the power is the same nearly in all the cases. If any thing, it should be less with the same pole than with the contrary. The reverse effect which appears is, I doubt not, due to the variation in the delicacy of the instrument. The use of longer conductors will take that effect away.



11734. In order to compensate this effect in a certain degree, the bar B was placed thus, so that its S end, being brought forward, might counteract the effect of the N ends on the needles. The needle was now less sensitive and the effects:

On	34	33	32	30	Average $32^{\circ}{\cdot}25$	mean $35^{\circ}{\cdot}12$
Off	40	37	38	37	„ $38^{\circ}{\cdot}00$	

Before (11731) with like poles near each other—it was $41^{\circ}{\cdot}25$.



11735. Placed the bar B with its contrary pole near the N of bar A, which would tend to increase the sensibility of the needles. The effects were:

On	40	40	41	38	38	Average $39^{\circ}{\cdot}4$	mean $42^{\circ}{\cdot}7$
Off	48	47	43	46	46	„ $46^{\circ}{\cdot}0$	

Before, it was $35^{\circ}{\cdot}12$.

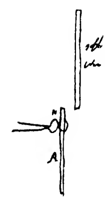
11736. So when $\text{N}\vert$ alone $= 37{\cdot}66$

$$\text{N}\vert\overset{\text{N}}{\overline{\quad}} = 41^\circ{\cdot}25 \text{ and } {}_{\text{N}}\vert^{\text{N}} = 35^\circ{\cdot}12$$

whilst $$\text{N}\vert\overset{\text{S}}{\overline{\quad}} = 34^\circ{\cdot}00 \text{ and } {}_{\text{N}}\vert^{\text{S}} = 42^\circ{\cdot}7$$

So the change of position of bar B just inverts the order of things —and the mean of N, N is 38°·18, and that of N, S 38·35, and the mean of the two 38°·26. So that it is evident the power of the N end of A was very little altered if at all by the vicinity of the same or the contrary pole of bar B.

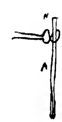
11737. The bar of soft iron was associated thus with bar A.



On	35	35	33	Average 34·3	mean 37°·3,
Off	39	40	42	„ 40·3	

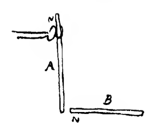
which is nearly the average of the former observations, only a little less. The iron bar drawing on to itself the power of the N pole of bar A, removed it from the needles and made them less sensible.

11738. Took away the iron bar, and the effect on the place of the needles and their delicacy was seen at once. Obtained results with the *bar A alone*.



On	30	28	31	29	30	Average 29°·6	mean 35°·7
Off	39	43	44	43	40	„ 41°·8	

11739. Associated bar B with the bottom of bar A.



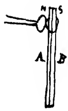
On	35	35	Average 35	mean 39·5
Off	44	44	„ 44	

The power of the N pole of bar A on the needles was probably increased by this arrangement.

11740. Thinking the equator of bar A might be lowered by this arrangement, I carried the loop a little further down, i.e. about an inch below the middle.

On	37	37	average 37°	mean 38°·25
Off	38	41	„ 39°·5	

The result is less: and when I made the journey still further down, the result was still less. The middle part or equator is the place to stop at.



11741. I now placed the two bars A, B, together with *unlike poles* in contact, and passed the loop over *both*.

On	2°	2°	mean 2°
Off	2°	2°	

So we see here how thoroughly the power of one is carried into and round by the other.

11742. Placed them with *like poles together*, to represent one double magnet.

On	38	40	38	39	average 36°·25 [*sic*]	mean 39°·87
Off	42	45	42	45	„ 43°·5	

So the two together only shewed 39°·87, and nearly one half of the power of each must have been crushed or compelled inwds. or carried off by the other, since it did not appear externally.

11743. The bar *A alone* gave

On	33	32	average 32·5	mean 33°,
Off	34	33	„ 33·5	

after the above experiment, proving that the magnets were not discharged, but opposed and for the time were subject internally to each other. This shews how collateral bars may add little or nothing to a magnet–and also affords a fine illustration of the inner and outer disposition of force.

11744. I must have very hard thin steel magnets–plates or wires.

11745. The joint observations to right and left by the contrary currents are very valuable, since if there be a displacement of the needle place or Zero, it then tells in contrary directions on the two sides and is compensated for.

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11746. Four steel bars, 12 inches long, 1 inch broad and 0·05 of an inch thick, have been made very hard–and then magnetized by the long bar magnets (11762). They are now marked C, D, E and F.

11747. Another loop has been prepared. The loop itself is 2·3 × 1·6 inches and is of copper wire 0·1 of an inch thick and $7\frac{1}{4}$ inches in length of wire (11755).

11748. The Galvanometer with thick wire has been arranged in

my room with new conductors of copper wire 0·2 of an inch in diameter. The length of these including the thick wire of the loop arms is 18 feet, and there are 24 inches more in the galvanometer coil. The center of the Galvanometer is now 9 feet from the place of the experimental magnets.

11749. When bar A was put into position with N end upward, the galvanometer needles required 27 beats of my watch to swing from 30° on one side to the other. When the S end was up, the time was 26 beats—so the bar has a little effect still, but it is very small and will be wrought out by taking the deflections on both sides.

11750. When the loop of 0·1 wire (11747) was in place, one of the bars C, D, E or F having the pole passed once in and left, gave a deflection of from 5° to 10°, according to the side it was sent to. This amount of effect will do. By using one of the bars A or B, it was seen that the increase in length of the conductor had diminished the sensibility of the instrument considerably.

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11751. The Galvanometer copper coil is magnetic and attracts the needle feebly, so that it will set either on one side or the other of Zero. This not only confuses the starting point but it adds a disturbing power to the effect of the earth and of torsion, which disturbs the forces through the first 10° right or left, and interferes with the measurement within such degrees.

11752. Took down the Galvanometer and went with sand paper and a stick all over the surface of the copper coil. Brushed it well with a camel hair pencil and remounted it, and now found the effect entirely removed, or so small a trace remaining as to be of no consequence.

11753. Still the needles did not stand in the Magnetic meridian, but 10° or 15° on one side of it. Found this was due to torsion in the suspending fibre. Yet there was not torsion due to one revolution, for when the needles were turned round once to relieve this torsion, it was created in the other direction, and now the needles stood away from the meridian on the other side. Undid the torsion above as far as I could with my arrangement and then set round the circle and coil until it corresponded with the needles.

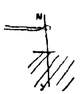
There was still some torsion. The effect of it is to make the swing on one side greater (i.e. when it diminishes the torsion) and on the other side less (i.e. when it increases the torsion). But this effect is eliminated and corrected by taking the swings in opposite directions, due to the contrary currents produced by cutting curves in one direction or the other, and halving their sum for the true expression of force.

11754. Hence a great value of the double observations.

11755. The loop I have used to-day is that before described (11747), with wire 0·1 thick. All the distances, etc. are as yesterday.

11756. First measured the power of the four hard bar magnets C, D, E, F (11746), passing the loop over a pole to the equator as before. This was done with both poles of each magnet upwd. in turn–to ascertain if they were alike in power, and as they stood nearly in the dip, to ascertain whether the earth's magnetism affected them.

11757. When the needle stands 1°, 2° or 3° on one or the other side of zero, which may sometimes be due to the arrangement of the magnets experimented on, it is better to mark this at the beginning and end of a series, and then having read by the graduation, to add and subtract this amount on the opposite sides.



11758. *C Bar*, thin hard steel (11746) with N end upwards.

Loop over the pole	5°	6°	7°	6°	Average 6°		mean 5°·37
Loop off the pole	5°	5°	5°	4°	„	4°·75	

C Bar. South end upwards.

Over	5°	5°	5°	5°	average 5°		mean 5·5
Off	6°	6°	6°	6°	„	6°	

This bar seems a little the strongest when the N end is downward, but the difference is very small.

11759. *D Bar* (11746). N end upwards.

Over	8°	8°·5	8°	8°·5	Average 8°·25		mean 8°·37
Off	9°	8°	9°	8°	„	8°·5	

S end upwards.

Over	8°	8°	8°	8°·5	Average 8·12		mean 8·36
Off	9°	8°	8°·5	9°	„	8·61	

The power of this magnet is higher than the other for I believe it is better hardened. It is the same whichever end is uppermost.

It is a good sample of the ability of measuring the force of a magnet.

11760. *E bar* (11746). N end upwards.

Over	9°·5	8°·5	8°·5	9°	Average 8·87	mean 9°·31
Off	9°·5	10°	9°·5	10°	„ 9·75	

E Bar (11746). S end upwards.

Over	8°·5	7°·5	7°·5	8°·5	average 8°	mean 8·25
Off	8°	8°·5	9°	8°·5	„ 8°·5	

So this bar is strong but the two results differ—9·31 and 8·25. The strongest effect is when the S end is downwards, and so as regards the supposed action of the earth is contrary to the former result. Probably the power is not uniform in its disposition. Can try it by filings.

11761. *F Bar* (11746). N end upwards.

Over	7°	6°·5	6°·5	7°·5	Average 6°·87	mean 7°·28	7°·14
Off	7°·5	7°·5	7°·5	7°·5	„ 7°·5		
S end upwards.							
Over	7°·5	7°·5	7°·5	7°·5	Average 7·5	mean 7°	
Off	6°·5	6°·5	6°·5	6°·5	„ 6·5		

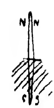
Here a weaker magnet and a little difference of the ends.

11762. So these four magnets appear to be as follows in power—

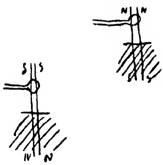
	°	
C	5·43	They are hard and we shall see how they can retain their power.
D	8·36	
E	8·78	
F	7·14	

11763. Took D and E as the strongest and put them with like ends together, so as to form a compound bar magnet, but varied the distance at times to see what was the effect as to their keeping their powers or quelling each other more or less; and first with ends close together but because of curvature open a quarter of an inch in the middle.

11764. *D, E.* North end upwards.



Over	16	17	18	18	average 17·25	mean 16°·81	16°·43
Off	17	16·5	16	16	„ 16·37		
S end upwards.							
Over	16·5	17	18	18	average 17·37	mean 16°·06	
Off	15	15	15	14	„ 14·75		



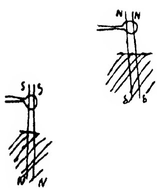
11765. Now the elementary plates D, E, were placed about 0·4 of an inch apart, but the loop of course passed over both.

D, E. North end upwards.

Over	15·5	14	14	14·5	14·5	average 14°·5	mean 16°·4	16°·6
Off	18·5	19	18	19	17	„ 18·3		

South end upwards.

Over	18	17	17·5	18·5	average 17·75	mean 16·81	
Off	16	16	15·5	16	„ 15·87		



11766. Placed the same bars nearly an inch apart and proceeded in the same manner, making the loop pass at once over both the upper poles.

D, E. North end upward.

Over	15	15	15	15	16	15	Average 15·16	mean 15°·37	15°·77
Off	15	18	16	15	15	14·5	„ 15·58		

South end upwards.

Over	17	16	16°·5	16	average 16°·37	mean 16°·18	
Off	16	16	16	16	„ 16°		

11767. There were some curious vibrations of the needle during the observations with the N ends upwards. I found that the sun had crept round the window and was feebly shining on to one of the mercury cups in which one of the connecting wires dipped, and that this caused a wavering thermo current; so sensible is the instrument. Shut off the sun and then all became right again. Have allowed for the effect by the double observation as well as I could.

11768. So the power of D and E when conjoined and close together is 16°·43

0·4 of inch apart 16·6, or nearly the same.

1·0 „ apart 15·77, or 0·66 below the case of close together.

D alone is 8·36 }
E alone is 8·78 } 17·14, i.e. supposing the larger and the smaller arc indicate strengths simply proportionate (11796, 7, 8, 819, 20, 1).

11769. The bars C and F were placed with like poles close together, and tried once with their North ends upwards.

Over	12°	12°	11°	12°	average 11°·75	mean 11°·37
Off	11°	11°	11°	11°	„ 11°	

C alone gave 5·43 } 12°·57, so that the difference is not great
F alone " 7·14 }
from the one effect when conjoined.

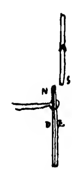
11770. Proceeded to subject the poles of a hard bar magnet in various ways to a more powerful magnet for the purpose of ascertaining whether the amount of force belonging to the hard bar was in any way affected, and if so whether the forces of the approximated bar were received into and united with those of the hard bar—or that the hard bar had as a system of forces its own independant fixed proportion. Employed the compound magnet consisting of bars D, E as the hard bar—and the bar Magnet A as the contiguous or modifying bar.

11771. Bar A with North end upwards.

Over	25°	25°	25°	25°	average 25°	mean 25°·62	25°·74
Off	27°	26°·5	26°	25°·5	" 26°·25		
	South end upwards.						
Over	26°	25°	25°·5	25°·5	average 25°·5	mean 25·87	
Off	26°	27°	26°	26°	" 26°·25		

11772. The compound bar D E gave as its power 16°·43 (11764) so that it is not above two thirds the power of the affecting or modifying bar.

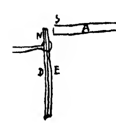
11773. First unlike poles near each other, but as in the figure, that as much air or space should be left about D E as possible for the free exertion of its action in that respect. The bars are 12 inches long and the poles were about 0·5 of an inch apart.



Over	18°	16°	16°	16°	average 16°·5	mean 16°·37,
Off	17°	16°	16°	16°	" 16°·25	

which is very nearly the same as the bar D E alone.

11774. Now bars arranged thus, the same poles being near each other as before.



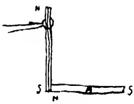
Over	16°·5	16°·5	17°	19°	17°·5	average 17°·3	mean 16°·4,
Off	15°	16°	16°	15°	15°·5	" 15°·5	

which is close upon the original bar D E.

11775. Brought the N end of the associated bar into contact with the S end of the chief bar D E thus—which is a position as likely to increase or add to the power of D E as any one.

Over	19°	20°	19°	19°	average 19°·25	mean 18°·75
Off	19°	18°	18°	18°	" 18°·25	

So here the power appears to have been raised a little by the influence of A bar.



11776. Now placed the bar A favourably at the lower end of D E.

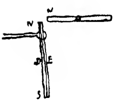
Over	17°	17°	17°·5	17°	average 17°·12	mean 17°·18;
Off	18°	17°	17°	17°	„ 17°·25	

has fallen towards its normal state, but is still helped a little.

11777. Passed through the same course but changed the pole of bar A, so that the latter should oppose or tend to reduce D E in power, and first for the position correspondant to that of (11773):

Over	15°	15°	15°	16°	average 15°·25	mean 15°·37;
Off	15°	16°	16°	15°	„ 15°·50	

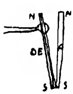
is reduced a little below the normal amount (16°·43).



11778. The next position was like that of (11774):

Over	16°	16°	16°	16°	average 16°	mean 15°·68;
Off	15°	15°·5	15°	16°	„ 15·37	

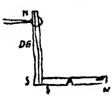
is nearly as the last.



11779. The next position as that of (11775):

Over	16°	17°	17°	17°	average 16·25	mean 15°·37;
Off	14°·5	14°·5	14°·5	14°·5	„ 14°·5	

nearly as the last two.

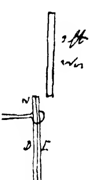


11780. The fourth position as (11776):

Over	17°	18°	16°·5	17°	average 17°·12	mean 16°·06.
Off	15°·5	15°	15°	14°·5	„ 15°	

Still a little below the normal (16°·43).

11781. Dismissed the modifying bar A and employed a soft iron bar of equal size in its place, being 12 inches long, 1 inch broad and 0·4 of an inch thick. It might be expected to raise the power of the bar D E somewhat.



11782. First position (11773):

Over	17	17	17·5	17	average 17·12	mean 16°·24;
Off	15	15·5	16	15	„ 15·37	

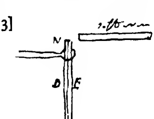
nearly up to the first normal state (16°·43).

11783*. Second position (11774):

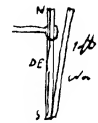
Over	17°	17°	16°·5	15°·5	average 16°·5	mean 16·43;
Off	16°	16°	16°·5	17°	„ 16°·37	

the same as the first normal state (16°·43).

* [11783]



11784. Third position (11775):



Over	17°·5	18°·5	18°·5	17°·5	average 18°	mean 18°;
Off	18°·5	17°·5	17°·5	18°·5	„ 18°	

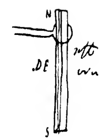
still higher and the highest.

11785. So the compound bar D E is depressed in power by opposing another and much stronger bar to it, and raised in power when this stronger bar or soft iron is presented favourably to it; but the change is but small, is very little as compared to the change with a bar of soft steel, and would perhaps be almost nothing with D E very hard.

11786. The average of the four results when D E is

opposed by the bar A is	15°·62
aided „ „	17°·17
aided by soft iron	16°·89
At the beginning the bar D E alone was . .	16°·43
The influencing bar had a power of . . .	25°·74

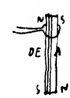
11787. In reference to the disposal of the external power of DE when in contact with soft iron or a magnet, it was put in contact with them and the helix passed over *both*.



Over	4°	4°	4°	4°	average 4·0	mean 3°·65
Off	4°	3°	3°	3°	„ 3·25	

11788. So a very large proportion of the power conducted through the iron, but still a certain amount conducted through the air or space, and probably as much as is proportionate to its magneto conducting force.

11789. Placed the bar magnet A in a similar relation to D E—



Over	12°	11°	11°	11°	average 11·25	mean 12°·5
Off	13°	15°	13°	14°	„ 13·75	

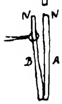
This 12°·5 is the surplus power of the bar A. The difference between it and D E is 9°·31 or 25°·74−16°·43—but then the bar A would rise in power by favourable association with the bar DE, and the more because it is not a hard bar but one easily affected (11817).

11790. It appears from the former results that the magnet A can affect the magnet D E a little, either in assisting or opposing it (11773–80), D E being examined *after* the bar A is in place. But the very affection of the external system of D E may be experimentally observed by putting the loop round D E first, and then

in thrice, the numbers were 25°, 24°, 25°. So that here the powers and the indications were well as 1, 2 and 3. There was hardly time to make four introductions in one swing, but the numbers were 29° and 32°—and I think the last is a true result and would appear again and again, if I were expert and did not loose power.

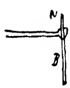
11800. To see how far a magnet is permanently, or for a time, depressed or raised when it is soft, I took bar B and ascertained its power alone when N end was upwards.

Over	16°	15°	15°	average 15°·33	mean 14°·83
Off	13°	15°	15°	„ 14°·33	



11801. Then associated A, whose power to-day is (25°·74), adversely with it thus; and ascertd. the lowered power of B.

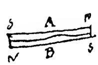
Over	7°·5	8°	8°	8°	average 7°·87	mean 7°·87
Off	8°	8°·5	7°	8°	„ 7·87	



11802. So it is reduced nearly one half *whilst it is in contact.* Then took away A and observed how much B was permanently reduced.

Over	12	13	13	13	average 12·75	mean 13°·06
Off	13	13	14	13½	„ 13·37	

So it rose a great deal, though not to the first state (14°·83), and the rise must have been from the resil[i]ence of power coerced for the time within bar B but not destroyed, and so was ready to come out the moment A was removed.



11803. Touched B by laying A favourably along side of it for a moment, to evolve any further power within it; and then removing A altogether, tried the power of B alone; it was now:

On	15°	15°	average 15°	mean 15·25
Off	15°	16°	„ 15·5	

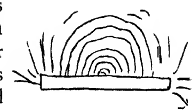
So that it was now raised even above its first state of (14·83).

11804. How important it is to have hard unchangeable magnets.

21 NOVR. 1851.

11805. Examined the four hard steel bars C, D, E, F (11746, 62) by filings. The display of power was very fine, being regular or nearly so in some but very irregular in others. E and F were the most regular in their arrangement of force. The lines of force had no reference to points that might be called poles, but issued

from the edges of the bars at every part up to the magnetic equators. The equators were not in the middle of the length, but in the bar E at 6·4 inches from the North end and therefore 5·6 inches from the south end; in the bar F the equator was 6·25 inches from the North end and 5·75 inches from the south end. The lines of force issued almost at right angles from the edge of the bars from the equator to within an inch or half an inch of the ends each way, and far more resembled parts of circles than hyperbolas, or peculiar magnetic curves, and that on both sides; and these curves were very beautiful indeed, extending three or four inches (and probably more if sought for) from the edges of the bars.



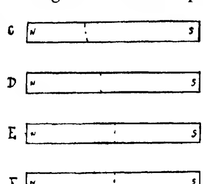
11806. The bars C and D were very irregular. Bar C had its equator about 4·3 inches from the north end and so 7·7 inches from the South end. The form of the curves dependant on the latter part was much disturbed, shewing that the generating force or condition *in* the magnet was irregular. The bar D had its chief equator about 5·3 inches from the north end and 6·7 from the south—it was also irregular—and *over* the bar were curious irregularities and places of greater concentration of force, shewing that even in the width the distribution was very irregular. All this was due probably to irregular hardening in the first place, and next, to irregular contact of the poles of the magnets when they were magnetized.

11807. Must harden the bars uniformly and well, and then magnetise them as uniformly in the magnetic helix, employing the half cores.

11808*. Very irregular forms C
Very irregular on the surface . . . . D
Very good forms . . . . E
Very good forms . . . . F

11809. To ascertain what degree of precaution was requisite in carrying the loop in experiments exactly to the same place near the magnetic equator of a bar, or what latitude of error might be allowed with a sensible effect on the results, the bar E was placed with the north end upwards, and the loop allowed to go more or less over the magnet in different experiments.

* [11808]



11810. First the magnet was blocked so that the loop descended over the pole to 6·1 inches below it; the results were:

Over	8°	8°	8°	average 8	mean 8°·16
Off	9°	8°	8°	" 8·33	

Then it passed downwds. 5·1 inches from the top.

Over	8°	8°·5	8°	average 8·16	mean 7°·74
Off	8°	7°	7°	" 7·33	

Loop over and down 4·1 inches.

Over	8°	8°	8°	average 8°	mean 7°·5
Off	7°	7°	7°	" 7°	

Loop over and down only 2·3 inches.

Over	6°	6°·5	6°	average 6·16	mean 5°·91
Off	6°	5°·5	5°·5	" 5·66	

Loop over and down as much as 8 inches.

Over	8°	8°	8°	average 8°	mean 7°·75
Off	7°·5	7°·5	7°·5	" 7°·5	

Loop over and down as much as 9 inches.

Over	7°	7°		average 7°	mean 6°·5
Off	6°	6°		" 6°	

11811. So for a journey of 2·3 inches below the pole the effect was 5°·91

"	"	4·1	"	"	"	7°·50
"	"	5·1	"	"	"	7°·74
"	"	6·1	"	"	"	8·16
"	"	8	"	"	"	7·75
"	"	9	"	"	"	6·50

So the maximum effect is clearly when the loop travels to the equator of this regular bar, and diminishes, as it ought to do, if carried either short of or beyond it.

11812. In experiments of yesterday (11791), it was assumed that the bar brought up outside of the loop at the equatorial part did nothing or nearly nothing. As the loop, in approaching by the equatorial plane, must intersect certain curves in that plane, and as the part of the loop which goes up to and touches the magnet must intersect more than the other part of the loop which represents the opposite side of the rectangle, so there ought to be a certain amount of effect. Therefore put the loop in connection with the galvanometer, left it quiescent; but holding the bar magnet A perpendicular with its north end upwards and so that

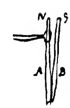
its equatorial plane should coincide with the plane of the loop, it was approached and removed, as between *a* or some further distance and *b*. When the magnet approached the loop, the indicating end of the needle went to the *right*; when the magnet receeded from the loop the needle end went to the left. The motion was very small, about 1°. If the magnet (always kept parallel to itself) was raised or lowered, and either its S end carried from above *into* the loop (or its N end from below), then a *great* and *contrary* motion of the needle was produced as expected.

11813. Now this amount and direction of the motion when the magnet was on the outside of the loop is the same as that before (11791) with the hard magnets. When however we come to the case of the softer and more changeable magnet (11792), then the quantities are so great as to shew the chief effect is due to an influence over the magnet itself which is within the loop.

11814. For a result of this kind in all its parts, put up bar A alone with north end upwards, and took its power by the loop.

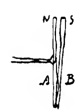
Over	23°	23°	22°·5	22°·5	average 22°·75	mean 22°·37
Off	22°·5	22°	22°	21°·5	„ 22°	

11815. Then associated bar B favourably with it thus:



Over	29°	28°	28°	28°	average 28°·25	mean 28°·12
Off	28°	28°	28°	28°	„ 28°	

11816. Now the loop was placed round the equator of A and fixed there, and then the bar B was brought up into the position represented and then removed, etc., and the effect noted.



When brought	8°	8°·5	8°·5	8°	average 8°·25	mean 7°·87
When taken away	7°	7°	8°	8°	„ 7°·50	

11817. The magnet A is increased in power from 22°·37 to 28°·12 by the proximity of B, giving a difference of 5·75. When the loop is about A, the bring. up B seems to change A, 7·87, being a difference of 2°·12. But the bringing up of B to the loop alone could affect it a degree or more (11812), and now we are to remember that when brought up, it is also strengthened by the proximity of A, so that its effect on the loop is proportionately greater. This is probably the cause of the difference of 2°·12.

11818. So here the change of the magnet is given in its two halves or component parts (11795).

11819. Repeated the experiments (11796, 7, 8) with one or more introductions of a standard magnet into the loop, using first for a small unit the magnet E, and afterwards the bars D E as one compound magnet. The north ends were introduced from above downwards as far as to the equator.

One introduction of E gave	8°	8°	8°	8°	average	8
Two " "	16°	16°	16°	15°	"	15·75
Three " "	23°·5	24°	24°	24°	"	23·87
Four " "	32°	31°	32°		"	31·66

The numbers are exceeding near to 1, 2, 3 and 4. The first number is most like to be wrong, because a small error of observation would tell quickly there. The last is the next like to be wrong, because it is difficult to make four successive actions within the necessary brief time;

but one half of the result with *two* introductions is	7·87
and one third of that with *three* " "	7·95
and one fourth of that with *four* " "	7·91

So there can hardly be a doubt but that the results are truly as 1, 2, 3 and 4; for if only 0·09 be subtracted from the first number or 8, it leaves that simple result.

11820. Combined D and E into one compound magnet for a higher unit of force; then:

one introduction of D E gave	15°	15°	15°	15°	average	15°
two " "	31	30·5	31·5	32°	"	31°·25
three " "	47°	47°	46°·5	47	"	46·87
four " "	58°	58°	58°	60°	"	58°·5

Here again the numbers are nearly as 1, 2, 3, 4, and the first and last numbers have to be allowed for as before (11819).

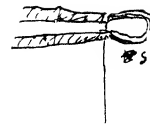
The two result halved is	15·62
" three " trisected is	15·62
" four " quartered is	14·62,

so that the first result increased by 0·6 would fall in with the second and third accurately.

11821. Hence the law of equal angular swings equal forces appears to be quite safe up to 45° or higher, and *also that the angular swings are directly as the forces.*

11822. So the reason why the two ends of a bar gave different amounts is not referable to the galvanometer ( ). Indeed the cause is now made manifest by the iron filings (11805).

11823. I have two good cylindrical bar magnets each 10·25 inches long and 0·85 in diameter, which are now well magnetized. They are marked G and H. I have prepared a loop on the usual thick wire conductors, but the loop is of copper wire $\frac{1}{20}$ of an inch in thickness and forms a ring only 0·7 of an inch in diameter. One of the round magnets, G, is placed with the south end upward and the loop is placed upon its end.

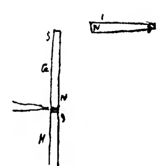


11824. When this loop is lifted off the magnet, the indicating end of the needle goes a little to the *left*. When it is put down again, it goes to the *right*. This is as it should be. The lines of force which rise from the part of the magnet where the helix rests and within when it is down upon the pole, are cut through very obliquely as it rises and again as it descends.

11825. Placed the second magnet H on to the top of the first, end on with unlike poles together so as to form a continuation of it, having the little loop in the middle. Putting on this magnet made the needle go to the right, and taking it off again made it go to the left.

11826. So putting the helix on to the top of S made the needle end go *right*, and putting the pole of the second magnet H on the top of the helix also made it go *right*. This should be so: the first action is like bringing up the S pole below the helix and the second the bringing up of the N pole above the helix, and both tend to draw the lines of force inward and *increase* the quantity flowing there through the magnets.

11827. Now arranged two other magnets at the top and bottom of the double magnet including the little helix, with contrary poles near the former, so that when approached they might increase the strength of the compound magnet G H. When these poles were brought up to and in contact with the former, then the needle end again went to the right, though only a very little. All these actions are consistent and right, and agree together.

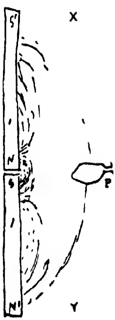


11828. When the poles N′ S′ were taken away, then the needle end went to the left.

11829. Now removed the little loop, which may be considered as within the magnet or at least as within the lines of force in the central or magnetic part of their course. Let the two magnets G H rest or touch each other, and put a larger or outside loop

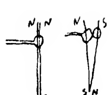
round them there, being that of 1·5 or 1·6 inches in width. Still the action of the two poles N′ S′ was the same as before. When they closed up to the chief magnets G H, the needle end went to the *right*; and when away, to the *left*, and more than before.

11830. I moved the helix to the equator of one of the magnets G; still the same effects were produced.



11831. A few filings shews the reason of all this. When the magnets are thus, the chief venters are at 1, 1, but there is a reverse venter at or between N, S, and whether it is the small helix above or the larger one (11829), it is within the lines of this venter. When S′ and N′ are reinforced by unlike poles approached to them, the lines of force at N, S, or this middle venter, are strengthened and so the effect is the same on both the loops involved in them. But when the loop is about 1 or higher up, and therefore in a venter having contrary direction of lines to the former, still the same effect is produced, i.e. the same current. But this venter is enfeebled when the poles S′ N′ are strengthened, for power which before proceeded from S′ over venter 1 towards N is now directed towards the *approximated* pole, and the power it engaged at N is now directed towards S to the strengthening of the forces of the middle venter; but all this weaken[s] the venter round 1, and hence the effect produced; for as the *direction* of the lines at this side venter is the *contrary* of that at the middle venter, so a strengthening of the one and a weakening of the other should produce currents in the *same* direction in wire[s] parallel to each other intersected by the lines of force there.

11832. If, at the equatorial plane passing between N, S, we wish to place a loop in the external lines of force, i.e. those proceeding from S′ to N′, then we must go a good way out beyond the neutral place. But if magnets are brought up at X, Y, to strengthen the middle venter, these disturb altogether the outer or external lines of force; in fact they pass into the newly approached magnets. Perhaps if the magnets were bent over thus, so that their outer ends might mutually help and strengthen each other, then a loop placed at N, S, and another at P would shew the proper effects and directions.



11833. If two quite unchangeable magnets could be prepared, then they should give the same result in power in either of the positions figured.

11834. *List of Magnets referred to.*

A. A bar, 12 inches long, 1 inch broad and 0·4 thick.

B. Do.

C. A plate, 12 inches long, 1 broad and 0·05 only thick, very hard

D. Do. (11808)

E. Do.

F. Do.

A cylinder, 10·25 inches long and 0·85 in diameter.

Do.

I. Do. 8·5 inches long and 0·75 in diameter.

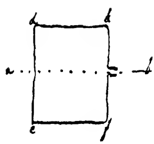
K. Do.

L.

M. See 11889.

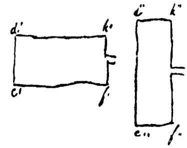
N.

11835. Proceeded to experiment on revolving rings and rectangles (11424, 11547) of copper wire under the influence of the earth's lines of magnetic force. Employed of course my thick wire Galvanometer and the commutator before described (11654), but the latter has been improved by having a cross frame attached to it at pleasure, which could carry rings a foot or more in diameter. Have also increased the pressure of the springs bearing against the commutator, which are now two upright copper wires 0·2 in thickness. The rings or rectangles when in place have their ends *soldered* on to the two parts of the commutator. The whole length of the connecting wire going to and fro between this apparatus and the Galvanometer is only 6 feet and is copper 0·2 in diameter—for now there was no occasion to go to a distance from the Galvanometer. The wires' ends are soldered—then amalgamated; then they enter into sockets very little larger than themselves, and there are pressed by screws, so that the junctions are all by mercury or soldering except at the bearings of the commutator. All acts exceedingly well.

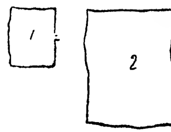


11836. Employed rectangles of wire instead of rings, as being more simple in their considerations. *ab* is the axis of rotation. Suppose such a rectangle to make one revolution; it is evident that if lines of magnetic force exist perpendicular to the plane from which it starts, that the parts *de* and *kf* will intersect no lines of force and therefore serve merely as conductors; that *dk* and *ef* will each twice intersect the lines within the rectangle *dkef*; also that they will do so in opposite directions, producing opposite electric currents, which will tend to run round the rectangle and out at the commutator *b*; and that as the direction of motion will change at each half revolution, so the commutator changes at the same moment and therefore the currents, though they intermit, always flow in the same direction to the galvanometer, for the same direction of rotation of the wire.

11837. It is also evident that, for the same length of wire and therefore for the same amount of obstruction as regards the conduction of these low currents, a square rectangle is the form which

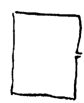


encloses the largest area and therefore the largest number or amount of lines of force; for though *d'k'* and *e'f'* take in a greater range of lines in one direction than before (11836), and though *d''k''* and *e''f''* travel with greater velocity than before, yet in neither of the latter cases are so many lines intersected as before, i.e. the inclosed areas are not so great; hence the first form or square rectangle should be the best.



11838. Again, the wire (of a given length) is twice as favourably disposed as one square rectangle than as a square rectangle of two convolutions. The rectangle 1 with 2 convolutions contains as much wire as the rectangle 2 with one convolution; but it contains only one fourth of the area and therefore, with the same resistance of wire, only evolves (according to expectation) half as much electricity. We shall see how this conclusion will turn out.

11839. Again, many convolutions of a thin wire cannot be equal to one convolution of a thick wire equal in mass to the many. Because in the former, the long thin wire offers great obstruction as compared to the short thick one, and there is no compensation for that obstruction in quantity of Electricity generated. As to intensity, probably also none, but we shall see by experiment.



11840. The first *rectangle* experimented with was a square of 12 inches in the side = area of 144 square inches. The wire was copper 0·05 of an inch thick and 48 inches in length. All acted well. The Galvanometer needle was close upon zero. *Six* revolutions gave 14° or 15° of swing, and 12 revolutions could easily be made before the swing to one side was half over.

Six revolutions–moderate	15°	15·5	16°	average	15°·5	15°·553 – 6 = 2°·592
" " quick	15°·5	15°·5	16°	"	15°·66	
" " slow	15°	15°·5	16°	"	15°·5	

so *very different velocities* gave very nearly the same results.

11841. Again.

Twelve rev.–moderate	29°	28°·5		average	28·75
" " quick	32°	31°	31°	"	31·33 ÷ 12 = 2°·61;

here the difference is evident–but I think it is due to the fact that the needle was so far deflected from the line of the coil by the first effect of the first moderate revolutions that the latter portion of the twelve had less power. Where during the whole *twelve* the needle was nearer to the coil, as when the time of

revolution was quick and short, so the full impact of force on the needle was more nearly obtained ( ). Hence quick motion should be best and also the number of revolutions not too many.

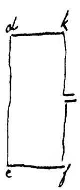
11842. Now for an estimate of the power, observation was made on both sides of 0°, that is both for *direct* and *reverse* rotation of the rectangle, and the number of revolutions was *nine*.

Nine revolutions	right 24°	24°	23·5°	24°	average 23°·87		mean 23°·62 ÷ 9 = 2°·624
	left 23°	23°·5	24°	23°	,,	23°·37	

So *six* revolutions give 15°·553; or 2°·592 for *one* revolution
twelve quick ,, 31·33; or 2°·61 ,, ,,
nine (both directions) 23·62 or 2°·624 ,, ,,

These results are beautifully near and proportionate, and shew finely that the electricity evolved is as the amount of lines of magnetic force intersected.

11843. They also shew as before (11796, 819) that the number of degrees of swing at the galvanometer is directly as the electricity which passes in this manner of application through it.



11844. Prepared a second rectangle, 8 × 16 inches, containing therefore 48 inches of the same copper wire as before of 0·05 diameter; but the intersecting parts *dk* and *ef* were eight inches in length and the are[a] of the rectangle over which they passed only 128 square inches instead of 144.

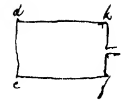
9 Revolut.	Right 21°	20°·5	21°	21°	average 20·87		mean 20°·56 ÷ 9 = 2·284
	Left 20°·5	20°	20°·5	20°	,,	20·25	

11845. As to variation of velocity:

6 Slow revolutions								
14°·5	14°·5	14°·5	14°·5		average 14°·5			2·416
6 quick revolutions							mean 14°·85 ÷ 6 =	
14·5	15°·5	15°·5	16	14°·5	,,	15°·2		2·533

11846. The smaller number of revolutions gives a higher and I think a better result than the larger, as before (11841). But the latter (Six) revolutions were all made to the right, and as may be seen above, it gives more than the mean. Still, the results are very well. The electricity produced is nearly as the areas, for:

144 : 128 : : 2·61 : 2·32, which is very close upon 2°·379, the mean of these experiments with the wide rectangle.



11847. Now prepared a rectangle of the same length (48 inches) of the same copper wire (0·05 in thickness), which was still 16×8 and therefore had an area of 128 square inches; but the intersecting sides *dk* and *ef* were now 16 inches each instead of 8. So they were twice the length they were before but had only half the velocity.

9 Revolns.	Right	21	21	20·5	20·5	average $20^\circ{\cdot}75$	mean $21^\circ{\cdot}06 \div 9 = 2{\cdot}34$
	Left	22	21	20·5	22	„ $21^\circ{\cdot}375$	

I suspect the centrifugal power made these long sides bulge outwards, which would increase the area of curves.

11848.

6 slow revolutions
13° 14° $13^\circ{\cdot}5$ average 13·5
6 quick revolutions mean $13{\cdot}67 - 6 = 2{\cdot}28$
14 14·5 14 14 13·5 13·5 13·5 „ 13·85

11849. So here the results may be considered as the same as for the former rectangle; for as then the final mean was 2·32, so here it is 2·31 for each revolution. Of course the accordance of the results to the area is the same. The Electricity is as the amount of curves intersected—all other things, as to length of wire, etc. being the same.

11849 *b*. Now prepared a square rectangle 12 inches in the side, of copper wire 0·1 of an inch thick and 48 inches in length. The area therefore was that of the first square rectangle, but the wire had *four* times the mass of the former.

6 Revol.	Right	42°	42°	42°	41°		average 41·75	mean $44^\circ \div 6 = 7^\circ{\cdot}33$
	Left	46°	47°	46°	46°		„ 46·25	
3 Revol.	Right	20°	20°	$19^\circ{\cdot}6$	21°	20°	average $20^\circ{\cdot}12$	mean $21^\circ{\cdot}61 - 3 = 7^\circ{\cdot}20$
	Left	22°	$23^\circ{\cdot}5$	$23^\circ{\cdot}5$	$23^\circ{\cdot}5$	23°	„ $23^\circ{\cdot}1$	

So these agree very close together.

11850. Three quick or three slow, always to the right, gave continually the same result, being 20° as nearly as might be. Perhaps the quick are a trace the best.

11851. Now with this rectangle the expression for one revolution is $7^\circ{\cdot}26$, whilst with a like rectangle of wire only half the diameter, or one fourth the mass (11841), it was $2^\circ{\cdot}61$. The increase with the mass is therefore very great and important.

11852. It is not to be supposed that the first number should be four times the second, because the conductors remain the same and the obstruction therefore increase. In former results with loops of copper wire of 0·05 and 0·1 in diameter, the proportions of the currents generated was as 16° to 44° (11660), which would be accordant with a ratio in the present case of 2°·61 to 7°·18; and it is in reality as 2°·61 to 7°·26—a very close approximation.

11853. Now observed the results with different numbers of revolutions of this Rectangle.

One Revolution	7°	7°	7°	7°	average	7° . . . 7
Two ”	14°	14°	14°	13°·5	”	13°·875 ÷ 2 = 6·937
Three ”	21°·3	21	21	21°	”	21°·075 ÷ 3 = 7·025
Four ”	29°	29°	28°	28°·5	”	28°·637 ÷ 4 = 7·159
Five ”	38°	38°	37	37°·5	”	37°·637 ÷ 5 = 7·527

These are indeed very close upon equality of curves intersected and electy. produced.

11854. A rectangle 12 inches square of copper wire 48 inches long and 0·2 of an inch in diameter—including therefore as before 144 square inches of area.

3 Revol.	Right	27°	25°	25°	25°	26°	25°·5	average 25°·58	mean 27°·04 − 3 = 9°·01
	Left	28°	29°	29°	29°	28°	28°	” 28°·5	

2 Revol.	Right	17°·5	17°·5	average 17°·5	mean 17°·75 − 2 = 8°·87
	Left	18°	18°	” 18°	

11855. When the two revolutions were made by fits or starts and by going on and back, irregularly, still when completed they gave the *same average result* of 17°·5.

11856. So the mean of this wire is 8°·94—or 1°·68 better than the last for each revolution.

11857. Before (11660, 2), the three wires 0·05, 0·1 and 0·2 in thickness when used as loops gave results as 1 : 2·77 and 3·58. Now in the form of rectangles subject to the earth's power, the results are as 1 : 2·78 and 3·45, which, considering the different forms of experiments and that they are the first, is astonishingly near.

11858. Formed a rectangle 12 inches in the side of copper wire 0·05 of an inch in thickness, of four convolutions; the area of the rectangle therefore is the same as before and the mass of metal the same as when a wire of 0·1 was employed, but the length is

four times 48 inches or 192 inches. This must no doubt increase the resistance, but will it get any gain in intensity so as to be more sensible to a thin wire galvanometer.

$$\begin{array}{l} 6 \\ \text{Revol.} \end{array} \left\{ \begin{array}{llllllll} \text{Right} & 20^\circ & 20^\circ & 20^\circ{\cdot}5 & 21^\circ{\cdot}5 & 21^\circ & \text{average} & 20^\circ{\cdot}6 \\ \text{Left} & 19^\circ & 20^\circ & 20^\circ & 20^\circ{\cdot}5 & 19^\circ & \text{''} & 19^\circ{\cdot}7 \end{array} \right. \quad \text{mean } 20^\circ{\cdot}15 \div 6 = 3^\circ{\cdot}36$$

11859. So the result is not half that of the former rectangle of equal mass (11851) or $7^\circ{\cdot}26$, though it does not differ far from it.

11860. I then employed these same two rectangles of equal mass and area but different diameters and lengths of wire with Rhumkorf's Galvanometer of fine wire. That of one wire 0·1 in thickness gave for

$$\begin{array}{l} 12 \\ \text{Revol.} \end{array} \left\{ \begin{array}{llllll} \text{Right} & 1^\circ & 1^\circ{\cdot}5 & 1^\circ{\cdot}5 & \text{average} & 1{\cdot}33 \\ \text{Left} & 2^\circ & 2^\circ & 2^\circ & \text{''} & 2{\cdot}00 \end{array} \right. \quad \text{mean } 1^\circ{\cdot}66 \div 12 = 0^\circ{\cdot}138$$

The other of four convolutions of wire of 0·05 gave

$$\begin{array}{l} 12 \\ \text{Revol.} \end{array} \left\{ \begin{array}{llllll} \text{Right} & 8^\circ & 8^\circ & 6^\circ & \text{average} & 7^\circ{\cdot}33 \\ \text{Left} & 7^\circ & 8^\circ & 7^\circ & \text{''} & 7^\circ{\cdot}33 \end{array} \right. \quad \text{mean } 7^\circ{\cdot}33 \div 12 = 0^\circ{\cdot}610$$

11861. There is therefore an immense falling off in both by the use of Rhumkorf's galvanometer as to the final result, i.e.

for the thick wire from $7^\circ{\cdot}26$ to $0^\circ{\cdot}138$
for the thin wire from $3^\circ{\cdot}36$ to $0^\circ{\cdot}610$;

but on the other hand the thin long wire has lost far less in quantity than the thick wire, and by this galvanometer is above four times greater, shewing the superior intensity it has in the rectangle. Of the first current, less than a *fiftieth* passes the fine wire galvanometer. Of the second, more than ten times the proportion, or between a fourth and a fifth, goes through it.

11862. See the value of the thick wire galvanometer here.

11863. Some smaller arrangements were now used, and first a ring *four* inches in external diameter made of one copper wire 0·2 of an inch in thickness. Thick wire Galvanometer.

$$\begin{array}{l} 6 \\ \text{Revol.} \end{array} \left\{ \begin{array}{llllll} \text{Right} & 7^\circ & 7^\circ & 6^\circ{\cdot}5 & \text{average} & 6^\circ{\cdot}83 \\ \text{Left} & 5^\circ & 5^\circ{\cdot}5 & 5^\circ & \text{''} & 5^\circ{\cdot}16 \end{array} \right. \quad \text{mean } 5^\circ{\cdot}995 \div 6 = 0{\cdot}999$$

final mean $1^\circ{\cdot}0$

$$\begin{array}{l} 12 \\ \text{Revol.} \end{array} \left\{ \begin{array}{lllll} \text{Right} & 13^\circ & 13^\circ & \text{average} & 13^\circ \\ \text{Left} & 11^\circ{\cdot}5 & 12^\circ & \text{''} & 11{\cdot}75 \end{array} \right. \quad \text{mean } 12^\circ{\cdot}375 \div 12 = 1{\cdot}031$$

11864. Then a ring having a diameter of 3·6 or 3·7 inches formed

of 300 inches of copper wire 0·04 in diameter, constituting 26 spirals in the ring.

12 Revol.	Right	5°	5°	6°	6°	average 5°·5	mean 6°·25 ÷ 12 = 0°·52
	Left	7°	7°	7°	7°	,, 7°·0	

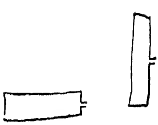
Here the masses are nearly the same, but the smaller wire is only $\frac{1}{5}$ the diameter of the larger one, or only $\frac{1}{25}$ of its mass as a single wire–it has with my galvanometer little more than half the power of the single thick wire.

11865. We know from former considerations that these 300 inches of wire would have been far better as one single ring or rectangle (11838).

11866. Could well apply the revolving rectangle to the investigation of different metals–or different masses–making the conducting parts *de* and *kf* of thick wire and the intersecting parts *dk* and *ef* of the wire to be examined.

11867. Apply it also to different thicknesses of the same wire.

11868. Apply it also to different lengths of the same wire.



11869. There appears to be no difference whether the lines of force intersected lie chiefly in the direction of the motion, and therefore are intersected in succession, or whether they lie chiefly in the length of the moving wire, and are therefore cut more at once than in the former case.

11870. A revolving ring or rectangle must be a very important examiner of lines of magnetic force–and small ones of single thick wire may be very useful in examining fields of varying action, as at different distances about magnetic poles.

27 NOVR. 1851.

11871. Have arranged a rectangle on a frame: it consists of 12 feet of copper wire 0·2 of an inch in diameter in a square rectangle of 3 feet in the side and so incloses an area of 9 square feet. It tells upon the Galvanometer beautifully and gives deflections up to 80° for a single revolution–and several good points were observed.

11872. Starting from the horizontal position and returning to it as in all former cases, it was easily seen that when the motion was uniform for the whole revolution the force at the Galvano-

meter on the needle was far greater at one time than another, being little for 10° or 20° of motion on either side of the horizontal plane but great for the same amount of motion on either side of the vertical plane, when the wire was cutting the curves directly across. The whole progress of the *two* impulses due to one revolution was indeed beautifully seen at the needle.

11873. When the rectangle was continued in its rotation even at a moderate rate, the needle was permanently deflected 40° or 50°. Shewing that the rectangle was as to quantity a good electrical machine, when good conductors were allowed.

11874. This rectangle being large and massive takes more time for a rotation than before. When a rotation was slow, the deflection was much less than when it had a certain degree of quickness. It was not easy to make one in a second. The most convenient quick speed, being an easy one, gave 80° for a revolution, whilst one of half the speed or less gave only 60°, 50° or less. Even with the quickest, the needle had left the plane of the convolutions considerably before the second impulse came on.

11875. Now whole revolutions were made in the quick fair time, sometimes right and sometimes left—the results are below:

Right 74 77·5 79·5 81 78·5 78·5 79 79 80·5 79·5 78·5 78·5 80·5 80 80;

the average of these fifteen observations is 78°·846.

Left 70 76 78 76 79 78 77 80 78 76 81 81·5 79·5 78·5 81 81 82;

the average of these seventeen observations is 78°·382, and the mean of the two is 78°·614.

11876. On examining the dimensions across the rectangle, I found it was not exactly 3 feet each way, being rather less one way. I corrected this and then had the following results:

1 Revol.	Right	80·5	80·5	80	81	81	81·3	average	80·71	mean 81°·44
	Left	82	83	81	81·5	82·5	83	,,	82·16	

This difference of 2°·83 I believe due to the correction of the area, or rather its enlargement to 9 Square feet.

11877. As the two impulses from the semi revolutions are different in their force upon the needle, because of the distance of

the latter from the coil when the second impulse comes on, so I observed the effect of only half a revolution, which would of course correspond with the first impulse when two are given in a whole revolution.

$\frac{1}{2}$ Revol.	Right	43	40	40	38	38·5	38·5	average	39°·66	mean 41°·37
	Left	43	44	44	42·5	41	44	„	43·08	

Now $81°·44-41°·37=40·07$ as the value of the second impulse, supposing the final numbers correct; and this different [? difference] of the two impacts or impulses:

$$\left.\begin{array}{ll}\text{1st} & 41·37\\ \text{2nd} & 40·07\end{array}\right\} = 81·44$$ is accordant with the effect that should occur.

11878. When a square rectangle of such copper wire of 0·2 of an inch in thickness was employed, area 1 sqr. foot (11854), the force per revolution came out as 8°·94, being the mean. The area of the present rectangle is 9 square feet and therefore *nine* times as many lines of magnetic force are here intersected. Now $8°·94\times 9=80°·46$, and in the present case one revolution through 9 square feet $=81°·44$, a coincidence so close that I have no doubt the difference is due to a want of accuracy in the dimensions of the areas. In fact, the $8·94\times 9=80°·46$ is less than the corrected large rectangle but more than it before the correction (78°·614).

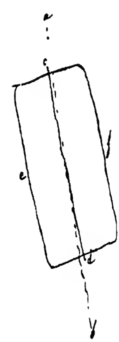
28 NOVR. 1851.

11879. In reference to the extent of swing on the Galvanometer and its proportionality to the electricity passing through it, I put up a magnetic needle as a torsion balance, using a fine glass thread as the filament; and then bringing the needle to its place in the magnetic meridian and the torsion index to zero, with of course no torsion on the filament, I deflected the needle through eight points in succession from 0° up to 90°, by torsion of the filament, observing the amount of torsion in each case for each point. My apparatus was not good, but in every case it required more torsion for the earlier points than the later ones. The average of several results was as follows:

Point			
Point 0	. .	0° of torsion	
			. . . 54·5
1	. .	54·5	
			. . . 61·5
2	. .	116	
			. . . 61·5
3	. .	177·5	
			. . . 59·0
4	. .	236·5	
			. . . 54·5
5	. .	291	
			. . . 43·5
6	. .	334·5	
			. . . 31·5
7	. .	366	
			. . . 21·0
8	. .	387	

11880. So the forces are probably very nearly the same for the first five points, or 56° or 60°, and afterwds. diminish. If the same result holds for an impact or brief push, then the indications of the Galvanometer ought to be nearly equal, for degrees up to 50° or 60°, and afterwds. gradually increase, for equal increments of force.

11881. In reference to the power of a revolving rectangle to shew when its axis of revolution is parallel to the Earth's line of force, by the absence then of any current produced in it, it would depend not upon the extent of the rectangle transverse to the axis of revolution but to the extent along the axis or parallel to it; for if *a b* be the line of force and *c d* the axis of revolution, it is the two small portions of the rectangle (or loop of any shape) between *c a* and *b d* that, moving in opposite directions when the axis and the line of force do not coincide, produces the current that is to shew they are not parallel. So it would not need a long rectangle, perhaps, but only a short one provided the axis were long. Still, a long one would give large development to the parts between *c a* and *b d*, and that might be requisite to gain intensity enough of current to indicate effects at the Galvanometer. But then the parts from *c* round by *e* to the line *b* ought to be massive to have their obstructing force removed from the current and also the corresponding parts on the other side.



11882. In order to judge what length of the parts between *c* and *a* and also between *b* and *d* would be necessary, I prepared a

ring of copper wire 0·1 of an inch in diameter, the ring being single and 1·2 inches external diameter. Being soldered on to the commutator (11654) and the latter turned by hand, I could easily obtain a deflection on either side of zero of 3° or 4°, employing the earth's force only as inducting power over the ring. My motion by hand was of course slow, but machinery could have given a revolution 30 or 40 times as quick and therefore more powerful. In order to reverse the effect on the needle, I had to reverse the motion, but in practice that could be far better done by a commutator in the course of the wires. Taking things however as they were, and considering that 2° on either side of zero is indication enough, then that would imply at least 0·6 of an inch for the distance between *c* and *a* and also between *b* and *d*—and if 0·6 is made to represent one degree of deflection from the line of the dip, then the axle *c d*, i.e. the diameter of the circle (216 inches) of which 0·6 is $\frac{1}{360}$ would require to be 68·7 inches, which is very long. But if a ten fold velocity reduced this to 6 or 7 inches, it would be nearer to a practical result.

11883. A second ring of the same wire but only 0·6 of an inch in external diameter—gave evident traces of action when on the commutator and turned by the hand.

11884*. If such a plan for ascertaining the dip should be useful practically, then one could perhaps make the axis a telescope and look at a distant scale to gain sensibility—using a reflector to turn the ray horizontal and so gain any amount of delicacy. The axis could rotate at one end, the lower, and be adjusted at the upper by two motions at right angles. Then, when rotating, the position above should be altered until the effect at the needle was a minimum, and when that was done, the adjustment at right angles should be altered until the effect at the needle was nothing. The axis would then be in the dip, and by looking through the telescope, the position could be read off on the distant scale.

11885. The magnet E (11805) gives lines of force of the circular form described, and these, if taken as they issue from the magnet

* [11884]

all round it, at a given distance from the end, as 3½ inches from the N or the S end, seem to be more abundant at the edges of the magnet than opposite the flat face; and so they ought to be, if the surrounding space or medium goes for anything—an analogy quite like that of the issue of electric currents from flat electrodes into a surrounding electrolyte existing.

11886. If poles be adopted as the representation of the forces, then I suppose that for this case an axial line from pole to pole ought to represent the resultant of distribution, and not the edges of the bar, or at least the edges should not do so much. But to ascertain whether they did do much, I fixed a copper ring on to the commutator (11654), consisting of wire 0·1 of an inch in thickness and the ring 1·1 inches in external diameter. This was placed as in former experiments, i.e. with the axis of rotation horizontal and the commutator changing contact when the ring passed through the horizontal plane; and this being understood, the circle in the figure may represent not this ring but *the direction in which it revolves*. Then the bar magnet E (11805, 8) was adjusted under the ring in such a position that the lines of force issuing from the parts of the bar about 3½ inches from the S end should so pass that the ring in its revolution should intersect them. Let the dotted line N S represent the axis of the bar: then in different experiments the bar was placed either edge on towds. the revolving ring or revolved 90 on the axial line N S, so as to present the side face; in all cases the same part of the bar being opposite to the revolving ring, and also the line N S at the same constant distance (1·2 inches) from the ring. The following were the results:

Bar edgeway to the Ring and 10 revolutions of the ring	Right	7°	7°	7°	average 7°	mean 5°·66
	Left	5°	4°	5°	„ 4°·33 [*sic*]	
Bar sideways to the ring and with 10 revolutions of the ring	Right	5°	4°·5	5°	average 4°·83	mean 4°·56
	Left	4°	5°	4°·5	„ 4°·3	

11887. So here is a difference as great as 4 : 5, which is I believe due to the position of the bar. There ought to be a difference of this kind in the idea of the necessity of the external medium. Should it also exist in the idea of resultant centers or poles?

11888. As arranged, the Earth's power would combine with the

experiment, but then it would be constant, and as seen before, it is of very small amount for 10° revolutions (11882); still, it should be eliminated. Whether it was with or against the difference I cannot tell now, because I do not know the direction of the solderings at the Commutator.

6 DECR. 1851.

11889. List of Magnets (11834).

A. Bar, $12 \times 1 \times 0{\cdot}4$: hardened at Yard furnace and magnetized by helix and two soft iron cores.
B. Do. . . . .
C. bar, $12 \times 1 \times 0{\cdot}05$: Do. . . . .
D. Do. . . . .
E. bar, $12 \times 1 \times 0{\cdot}05$: hard and as before (11834).
F. Do. . . . .
G. Do. . . . . hardened and magnetized as A, B, C and D.
H. Do. . . . .
I. Cylinder, $8{\cdot}5 \times 0{\cdot}75$: as from Newman.
K. Do. . . . .
L. Do. . . $10{\cdot}25 \times 0{\cdot}85$ Do. . . . .
M. Do. . . . .
N. bar, $6 \times 1 \times 0{\cdot}4$: hardened at hot air furnace and magnetized as A, B, etc.
O. Do. . . . Do. . . . .
P. Do. . . . Do. . . . .
Q. Do. . . . Do. . . . .
Ḣ. bar, $6 \times 0{\cdot}5 \times 0{\cdot}1$: very hard, from Scoresby. Magnetized by cores and helix.

By filings, A, B, E, F, N, O, P, Q and Ḣ very irregular. C, D, G and H irregular, with breaks and uncertain poles.

11 DECR. 1851.

11890*. Round a piece of soft iron wire as core wound a fine covered wire as a helix; then placed it under paper. Sprinkled filings over it and magnetized it by a current varying from 1 to 10 pair of plates of Grove's battery. The strength of the magnet varied much with the force of the current—but the *forms* of the curve did *not seem to change*.

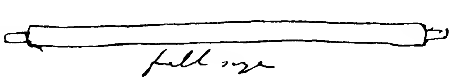
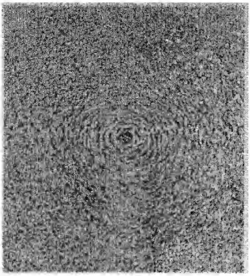
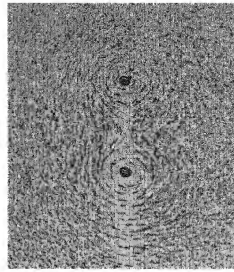
* [11890] 

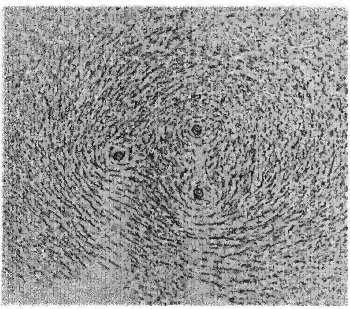
PLATE II



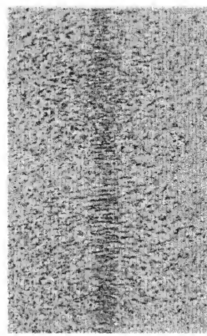
Par. 11891. 1 wire



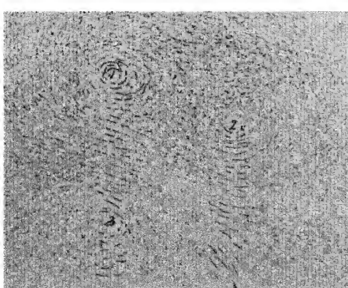
Par. 11893. 2 wires—like currents



Par 11895. 3 wires—like currents



Par. 11896. Lines across a single wire



Par. 11895. 3 wires—1 and 2 like currents,
3 contrary current
(*all three quarter scale*)

11891. A wire was arranged perpendicularly and the Current of 10 pair of Grove's plates sent through it. Filings sprinkled on paper assumed the form given in the following attached experimental result [Plate II].

11892. Wires of three different thicknesses were used, but there was no difference in the results. The power or rather the extent of the curves appeared to depend upon the amount of electricity in the current.

11893. When two wires carrying like currents were employed, the form[s] were very beautiful, shewing the coalescence of the lines of force, the inflection between the two, etc. etc. [Plate II].

11894. It appeared as if the two wires had twice the force of one wire, though the forms coalesced. As if the power of one was added on to the other and with no disappearance of the power of either. It was not a mere coalescence as in magnets, but the force was a sum of the two. There must be a neutral place between the two wires.

11895. With three wires carrying like currents the result was similar, as in the illustration [Plate II]. When one of the three currents was in the contrary direction, then the neutral place disappeared and the lines flowed between it and the others [Plate II]. Shewing the element of the helix.

11896. The lines over a current are given in the figure [Plate II] and are of course transverse to it.

11897. It is easy to trace and connect all these lines with those in the helix. Such as are produced over a ring helix are given in the side illustration[1].

11898. I have a helix made of 36 spirals of copper wire $\frac{1}{6}$ of an inch in diameter. The wire is not covered and the spirals do not touch; its length is 9 inches and its internal diameter 1·8 inches. It is easy to obtain access to the inside and, by placing a wooden half core within, the disposition of the lines in a plane passing through the axis of the helix could be shewn by filings both within and without. The opposite illustration, A [Plate III], shews the forms taken by filings both within and without about the end of the helix.

11899. Except near the ends, the lines within the helix were

[1] Not reproduced.

parallel or nearly so to each other from end to end, but when a core of soft iron about $2\frac{1}{2}$ inches long and $\frac{1}{8}$ of an inch thick was within, it disturbed the forms as in illustration B [Plate III].

11900. A still large[r] core, being a soft iron rod 5 inches long and 0·3 in diameter was introduced. C [Plate III] shews the result as to one of the extremities and the middle of the piece; the other end could not be conveniently recorded but was as the one rendered visible and as the result B. It is evident that the iron does not merely conduct the lines of force from the helix but is a new and abundant source of them. Those of the helix and the core associate beautifully together.

11901. When the same core was out of the axis and against the inside of the helix, then the result was as in D [Plate III].

11902. When the great helix alone was employed, always with the 10 pair of Grove's plates, and the form outside assumed by filings was observed, the lines were feeble, but as in the figure, where the intersection of the coils by the plane passing through the core axis is indicated—E[1].

11903. But when the core of soft iron 5 inches long and 0·3 in diameter was in the inside of the helix, then a great difference resulted; many lines of force issued through the side of the helix and the place of the magnet was easily seen by the place of the magnetic equator—F[1].

11905[2]. When two such cores were employed together, either side by side or end to end, there was much increase of power, and the lines shew where and its direction—in G[1] and H[1].

11906. A similar result is shewn in I [Plate IV], where a larger core was used.

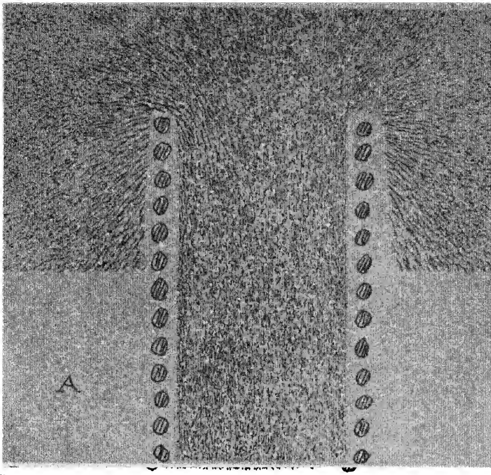
11907. Results with a helix consisting of four superposed and close together shew similar and correspondant results. See [K (not reproduced) and L (Plate IV)].

11908. I placed a bar of iron *outside* the helix—it was not absolutely insensible, for some of the lines of force from the ends of the helix feebly bent down towards it, but there was scarcely a trace of power. This has been stated before. See M[1].

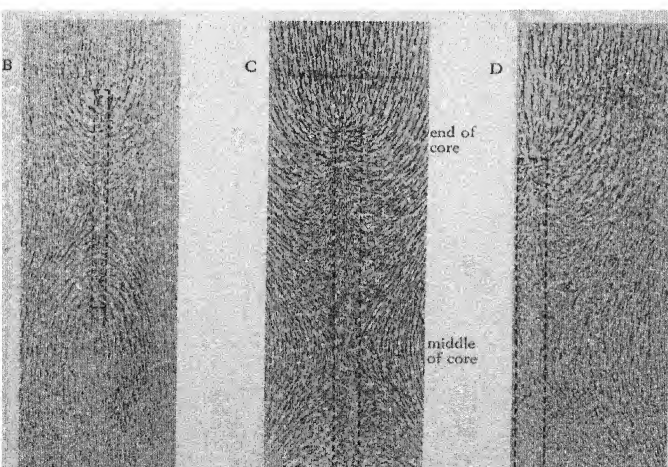
11909. Taking a long piece of soft iron wire 9·3 inches long,

[1] Not reproduced.

[2] There is no 11904 in the MS.



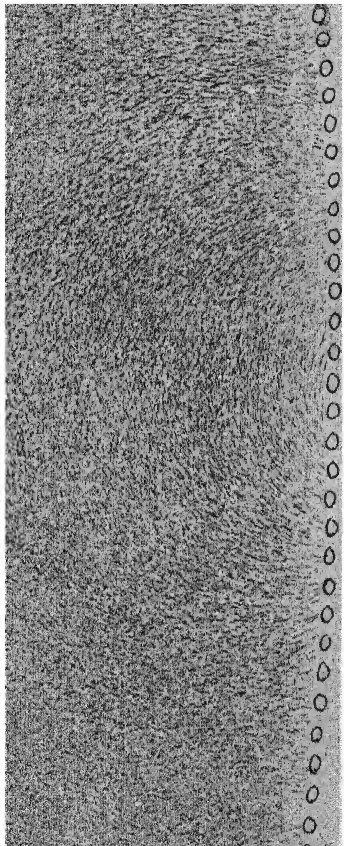
Par. 11898. A. Lines at the end of the large wire helix within and without—in a plane through the axis



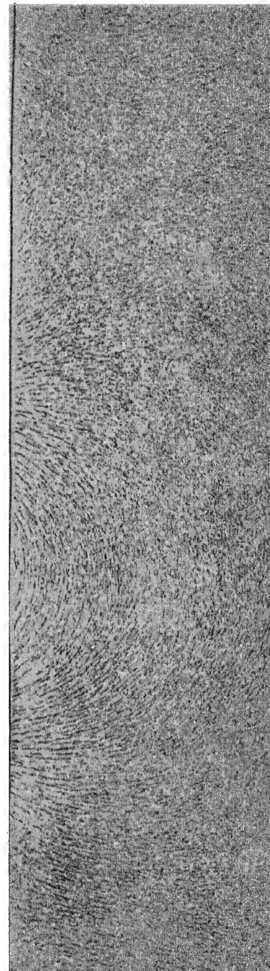
Par. 11899. B. A little core quite inside the helix
Par. 11900. C. Core of soft iron 5 inches long by 0·3 wide in the inside of the large helix
Par. 11901. D. Same core in the helix—at the side

(*The outlines of the iron cores, shown dotted here, are drawn in ink on the backs of the specimens. All seven tenths scale*)

PLATE IV



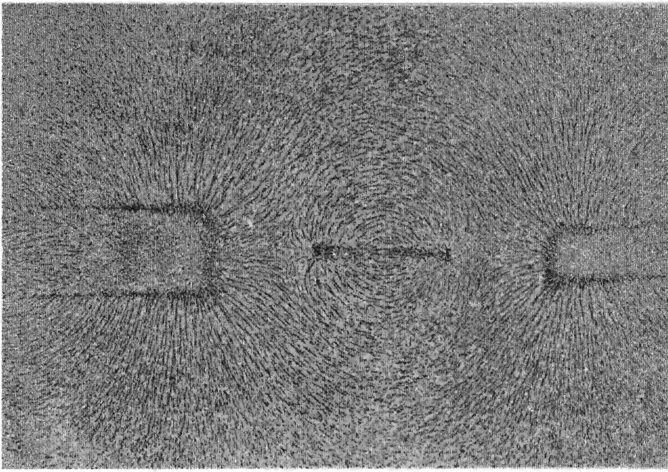
Par. 11906. I. Core—square bar soft iron 4·1 inches long and 0·5 thick



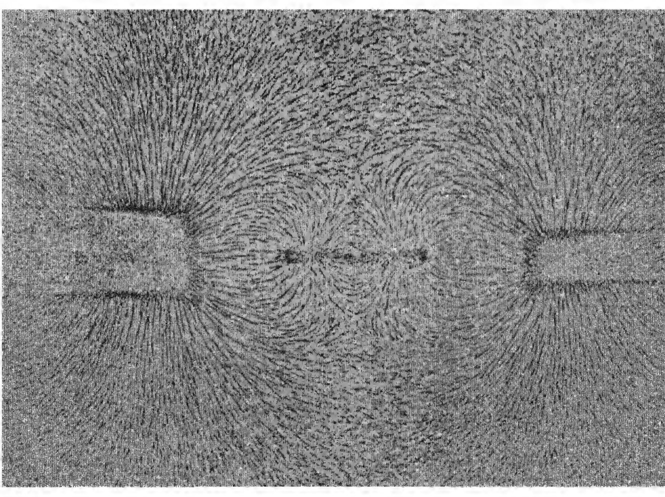
Par. 11907. L. Leather helix—core inside soft iron, 4 inches long and 0·3 diameter

(*two thirds scale*)

PLATE V

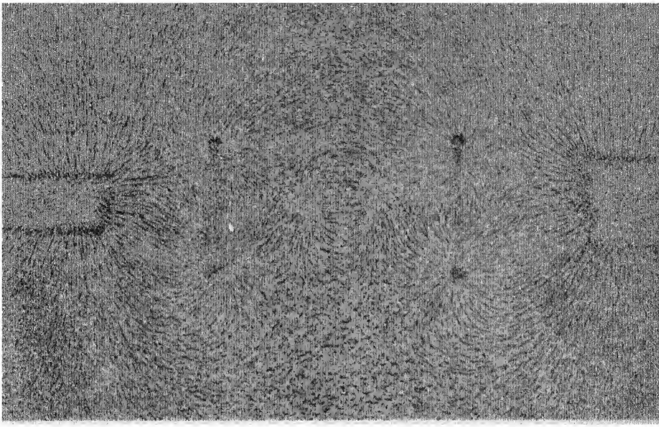


Par. 11911. R. (*two thirds scale*)

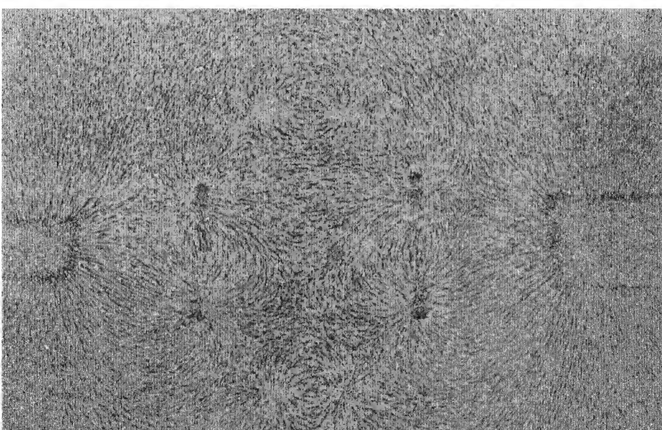


Par. 11911. S. (*two thirds scale*)

PLATE VI



Par. 11911. T. (*two thirds scale*)



Par. 11911. U. (*two thirds scale*)

I put round the middle of it a helix of covered wire which was 2·2 inches only in length and at the middle; see N[1]. When the current passed through the helix, all the external parts, being $3\frac{1}{2}$ inches on each side, became magnet., not with consecutive poles but simply and consistently as if the magnetism there pervaded outward.

11910. Cut away more and more of the iron. The concentration of force is seen in O[1], P[1], Q[1].

11911. Some combinations of the power are shewn in R, S [both Plate V], and also in T, U [both Plate VI] and V[1].

11912. T, U and V, some further illustrations of the power and the lines, some beautifully complicated.

16 DECR. 1851.

11913. Tried Scoresby's hard bar magnet under the influence of other magnets as to its change of condition. For that purpose ascertained the power of the bar magnet B, 12 inches long, 1 inch wide and 0·4 thick, which has been hardened and magnetized regularly by a helix and half cores. The loop when passed

On	40°	40°	40°	average 40	mean 40°·21 for Magnet B.
Off	39°·8	41°	40°·5	„ 40·43	

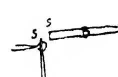
11914. The two magnets E F together as one compound magnet:

On	12°	11°·8	11°·5	average 11·73	mean 11·73 for Magnet E F.
Off	12°	11°·8	11°·5	„ 11·73	

11915. *Scoresby's Magnet*, 6 inches long, 0·5 broad and 0·1 of inch thick:

On	7°	7°	7°·5	7°·3	average 7·2	mean 7·04. Scoresby Magnet.
Off	7°	7°	7°	6°·5	„ 6·87	

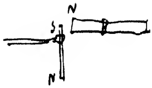
11916. Then Scoresby's magnet left unmoved, but the magnet B of nearly six times its power was placed adversely to it, as in the figure—the results were:



On	6°	5°·5	average 5°·75	mean 5°·87
Off	6°	6°	„ 6°	

So the Scoresby is here reduced from 7°·04 to 5°·87 by the opposed influence of this strong magnet.

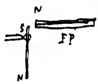
[1] Not reproduced.



11917. Turned B end for end, so as to give a favourable position and now:

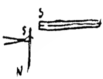
On	8°	7°·5	7°·5	average 7°·66	mean 7°·53
Off	7°·5	7°·2	7°·5	„ 7°·40	

So here the Scoresby is raised from 7°·04 to 7°·53. The elevation is not so much as the depression, nor was it to be expected.



11918. Now employed the weaker magnet E F of 11°·73 power to affect the Scoresby, which to begin with was 7°·04 power, and first favourably:

On	7°	7°	7°	average 7°	mean 6°·66
Off	6°·5	6°·5	6°	„ 6°·33	



Then unfavourably, as in the figure:

On	7°	7°·2	7°	average 7·06	mean 6·51
Off	6°·2	6°	5°·7	„ 5·96	

On trying the Scoresby alone, it was then found to be

On	7°·2	7°	7°	7°·3	average 7·12	mean 6·88
Off	6°·8	6°	7°	6°·8	„ 6·65	

11919. So that it would appear that the Scoresby had not been affected by the magnet E F.

11920. Taking a steel plate magnet, hard, 12 inches long, 1 inch broad and 0·05 of an inch in thickness, which had been magnetized highly in the helix by cores drawn along it when in the axis, I examd. it by filings and found it somewhat irregular but strong. Then ascertained its power by the loop when the N end was uppermost.

On	6°	6°·2	6°	6°·4	average 6·15	mean 6°·9
Off	7°·6	8°	7°·5	7°·5	„ 7·65	

11921. Broke it into two pieces very nearly at the middle--the power of one half was:

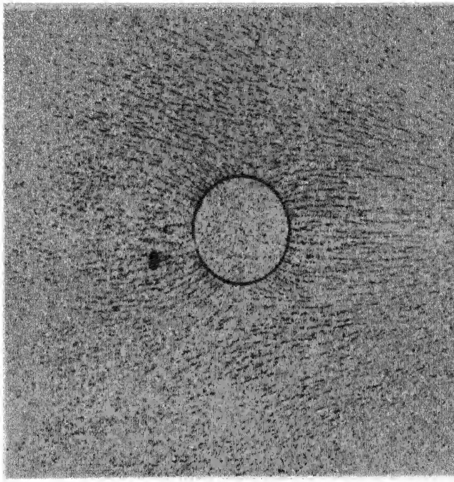
On	6°	6°	6°·2	average 6·06	mean 5°·94
Off	6°	6°	5°·5	„ 5·83	

The other half was:

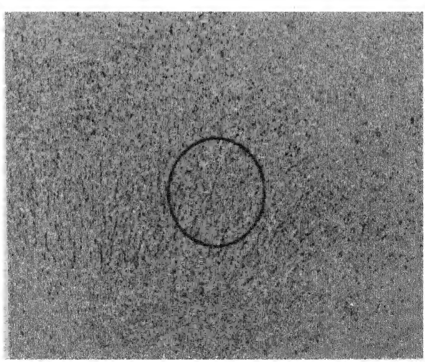
On	6°	6°·2	6°	average 6·06	mean 5°·89
Off	5°·6	5°·6	6°	„ 5·73	

11922. So the two pieces were each not much below the original magnet, being 5°·89 and 5°·94 and it 6°·9.

PLATE VII



Par. 11928. [C]



Par. 11928. [H]

(The circular outline of the nickel is drawn in ink on the specimens. Full size)

11923. The two pieces were placed together with like poles, so as to make a compound magnet; the joint power was:

On	11°·2	11°·5	11°·2	11°	average 11°·22	mean 11°·06
Off	11°	10°·8	11°	10°·8	„ 10°·90	

And the sum of the two separately is 11°·83, or very little more.

11924. The second half above was reduced by a horse shoe magnet until it[s] power was small; thus:

On	3°	2°·8	3°	2°·8	3°	average 2·92	mean 2°·91
Off	3°	3°	3°	3°	2°·5	„ 2°·90	

11925. This half of 2°·91 power and the remaining former one of 6°·9[1] power were put together as one compound magnet—the power came out:

On	8	8·5	7·5	8	average 8°·0	mean 7·83
Off	7·4	7·5	8	7·8	„ 7·67	

So they had affected and reduced each other, for the sum of their separate powers is 9°·81.

11926. Being placed with unlike poles together, the power was:

On	3°	3°·1	3°·4	3°	3°	average 3·1	mean 3·01
Off	3°	2°·8	3°·2	3°	3°	„ 3·0	

11927. I did not take the two separately again and cannot say how much they have been changed by juxta position, but the difference between their original forces is 4° very nearly.

20 DECR. 1851.

11928. N is the end of a bar magnet sending lines of force outwards; A is a hemisphere of nickel with its flat face upwards and a retort ring round it at some distance to support the paper on which filings are to be strewn. Whilst in the ordinary state, the form given to the filings was as in the figure, C, below [Plate VII]. On making the nickel hot by a spirit lamp, I was able to take away the spirit lamp, place paper over the nickel, sprinkle filings, tap, remove and place another paper for filings, etc., three and even four times before the nickel resumed its magnetic state. In this case the filings were in no way affected by the nickel, but just as they are in the illustration marked H [Plate VII] for hot; but going on to take successive forms of the lines, at a certain instant the lines became curved just as in the illustration C above.

[1] ? in error for 5°·94. See par. 11921.

11929. Magnetic effect of Sul. Iron or Oxygen.

With the hope of making an apparatus that might shew the effect of oxygen, constructed the following apparatus. *a b* is a fine straight slender filament of glass, suspended by a cocoon thread to the end of the wire *c*, which going through a cork, is by that held fast in the mouth of a compound tube, for *g* and *h* are two glass tubes connected by a copper socket at *s*. Now the glass filament is 12 inches long and its suspension thr[ea]d perhaps 16 inches in length. At the top and bottom of the glass filament is attached a small magnet, one at each end. A fine needle was well magnetised, then broken up into equal pieces about $\frac{1}{8}$ of an inch long, and there are two of them. They are attached in contrary directions to the filament and thus make something like an astatic needle, except that, being not quite in the same plane but thus ⪥, the apparatus does not point in the magnetic meridian but across it, with the marked ends towds. the north. This did not interfere with the expected action. A small mirror consisting of a piece of the silvering stripped from a mirror was fixed on at *m* to the middle of the glass filament, and this was brought opposite a notch cut in the copper socket, so that a ray of light could pass in and after impinging on the mirror, could be reflected out again to a telescope and observer; so that any degree of motion in the whole system could be observed.

11930. Now this apparatus being supported vertically so that the needle system was free to swing, it took up its place under the earth's influence. The needle was very delicate in its indications towd. or with any ordinary magnetic body. For the finest observations a candle was placed five feet off and its image in the reflector observed by a telescope. The image was bad but quite sufficient for the purpose.

11931. A bottle of crystals of proto sul. Iron shewed no effect on this needle by the earth's action. No signs of its conducting power or polarity or conduction polarity.

11932. A Jar of solution of Proto sulphate of iron (saturated),

being $3\frac{1}{2}$ inches internal diameter and 13 inches deep—did not affect it.

11933. A Glass bottle $6\times 6\times 7$ inches in size, being filled with the solution, did not affect it.

11934. Thes[e] things were tried in various positions, but no power of deflecting the lines of force of the earth appeared; there was not force enough for that, and of course there could not be any hopes of oxygen.

16 FEBY. 1852.

11935. I have prepared another magnetic needle. It is part of a needle, broken out of the middle after magnetization, and is not longer than this  : it is fixed on the back of a small piece of silvered glass about this size  and suspended by a single cocoon fibre in the square glass cell and tube of former times (11194, 249), from the end of a wire passing through the cork. A candle flame is well seen in this reflector by the telescope used on former occasions (11194, 249), and by means of the telescope wire, the position of the mirror (and needle) can be most readily ascertained and any change in position taken note of. The image of the candle flame was very good (11930).

11936. Having a large roundish block of hæmatite weighing 14 lbs., and about 6 inches in thickness every way, it was employed to see whether it would make a sensible deflection of the lines of magnetic force of the earth—a small piece being absolutely insensible in its action. It was therefore placed to the east and to the west, above and below the needle, at various times for this purpose being sustained on a table standing on a stone floor, whilst the needle was sustained on the end of an arm on another stand on the same stone floor. No mechanical disturbance of the needle occurred therefore when moving the heavy hæmatite by its mere weight.

11937. The hæmatite deflected the needle differently in different positions, and this shews a ready sensibility in the needle and goodness in the observing apparatus. The apparent motion of the reflected image to the right or left of the observer is, for the telescope, the *reverse* of that for the naked eye—the telescope being an inverting instrument. When therefore the image in it goes to

the right hand, it is really going to the left as seen directly by the naked eye. This correction is needful in determining the way in which the mirror and the needle moves.

11938. But the hæmatite proved to be slightly permanently magnetic, and therefore did not give any clear evidence of its affecting the terrestrial lines of force or becoming a magnet sensibly by position.

11939. A Winchester quart bottle of proto sul. Iron solution shewed no trace of magnetic effect by position. Was indifferent as on the former occasions.

11940. A remarkable proof of its sensibility occurred and the influence of distant things. It was on the basement floor between the Laby. and the street. Carriages passing in the street would be about 8 or 9 feet above the level of the needle and from 20 to 25 feet West of it at the nearest. As the carriages or carts passed, they affected the needle, and that in a constant manner; for whether they came up from the North or from the South, the S end of the needle went a little west and returned to its natural position as they passed away. If it was a quick passing carriage the action was quicker but quite steady, just one to and fro. If it was a slow moving cart or waggon, it was a slower motion but not a steadier one. When the vehicle was on the near side of the way the action was greater than when on the other side farther off. Considering every circumstance, I referred it to the action of the iron tires of the wheels, chiefly, the whole iron of the passing vehicle however combining in the effect. It is a magnet by position, i.e. it deflects the lines of terrestrial magnetic force, and this deflection was shewn by the needle, the motion of which is precisely that which would be produced by such an action.

11941. Try such a bar, and if there be an effect, consider how dilute the iron would be by expansion into a cube of 40 or 50 feet in the side, and whether if so diluted a small portion would be sensible at the needle. Thus a bar 4 feet long and 1·5 inches square occupies 108 cubic inches. A cube 40 feet in the side occupies 110592000 cubic inches. Now $\frac{108}{110,592,000} = \frac{1}{1,024,000}$. So that the iron would be above a million times diluted in that space, and a cubic inch of it so diluted would contain only the $\frac{1}{110,592,000}$dth part of the iron. Now a cubic inch of iron weighs 1969 grains, and

108 c.i. = 212,706 grains. The $\frac{1}{110,592,000}$dth part of 212,706 grains is rather less than the $\frac{1}{2000}$dth of a grain. Then query would the $\frac{1}{2000}$dth of a grain of iron, formed into a wire whose thickness should be $\frac{1}{32}$ of its length, and placed half an inch off from the needle, affect it as much as the bar 20 feet off (11959).

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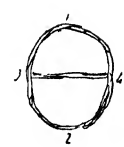
11942. Want to try a mass of Proto sulphate of iron, either as crystals or in solution, to ascertain whether it will sensibly affect the direction of the lines of force of the earth--that I may approach a step to the action of oxygen; for moderate masses of Sul. Iron do not sensibly affect a magnetic needle, though they are affected by a magnet (   ).

11943. 10 oz. avoirdupoise of small crystals proto sul. iron shaken together occupy about 15 cubic inches. The same quantity of crystals would make about 35 cubic inches of cold saturated solution.

11944. Have prepared a cask. It has wooden hoops. I have taken out all the *iron* nails and replaced such as were needful by copper nails. When standing up on end, the height within is 22 inches and the average diameter 16·875 inches, so that its capacity is nearly 5000 c.i. or 18 gallons. A loose division of wood has been made vertically down the middle to confine solid crystals to one side, if needful, and it has been marked 1, 2, 3, 4, on the sides to identify these sides when required.



11945. The needle and its reflector (11935) were first arranged on its separate stand on a stone floor, at the proper height, and also the candle, 73 inches off, to give an image in the reflector and the telescope, eye end 20 inches off, to observe. The needle was of course in the magnetic meridian within the room; the telescope and candle were west of it as in the next Figure.

11946. A travelling stool or platform was prepared to carry the cask. This by moving on the floor brought the top of the cask level with the magnetic needle, or being placed on a table where it could move 12 inches or more, then the bottom of the cask was level with the needle. This platform and the cask were to the East of the needle, and moving east and west could be brought up

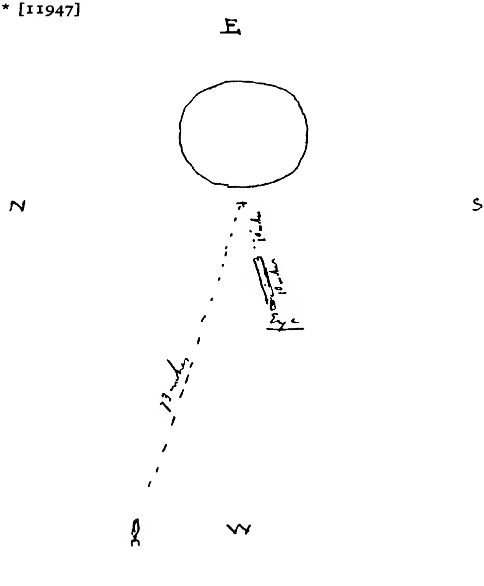
within 1 inch of the needle, or when on the ground taken feet off.

11947*. The Telescope inverts the object seen, but experimentally, whilst it and the candle are stationary, the movement of the image *in the telescope* to the *right* of the observer at Eye occurs when the S end of the little magnetic needle goes *Eastward*, and the motion of the image in the telescope to the *left* coincides with the movement of the S end of the needle to the *West*.

11948. First the Empty cask with its partition and supporting stool or platform was tried, the top being level with the needle. It was brought up to within an inch of the needle, or taken 52 inches off, always on the East of the needle—a careful observation was made in each position, and this was done for the four sides or faces of the cask (11944). With side 1 near or towds. the magnetic needle there was no change in the position of the needle, no deflection when the cask was nearest. With side 4 towards the needle, the same indifferent result. Sides 3 and 2 gave the same indifferent result. So the cask and stool or platform unexceptionable whilst below the level of the needle.

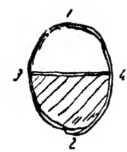
11949. The empty cask was now raised on the table (11946) so that its *bottom* was level with the needle—in this case it could travel 12 inches to and fro—its different distances being 1 and 13

* [11947]



inches. Now the side 3 gave a slight deflection of the image in the telescope to the apparent left = to about half the width of the candle flame. The sides 1, 4 and 2 gave the least possible deviation in the same direction when the cask was at the nearest. This looks like a trace of repulsion on the S end of the needle. It should have been the *contrary*, if the cask had acted in the *manner of iron*. Perhaps it was something in the travelling stool, in which there were iron screws, etc. It was very small.

11950. Crystals of Sul. Iron proto, 112 lb. in weight, were now put into the cask on one side of the division and just filled it to the top on the side 2, thus; and the whole being on the table and so mounted above the level of the magnetic needle, was experimented with, the distance of motion to and fro being 12 inches.



11951. Now when side 2 was near the magnetic needle, the image in the telescope went to the *left* distinctly, about half a flame width. When the cask was removed 12 inches further away, the effect ceased – the effect occurred again and again. With side 3 towards the needle the same thing occurred. With side 1 also the same – but the effect better, though now the sulphate is farther off. With side 4 the same thing. So the general result is that the Sulphate of iron causes the S end of the needle to go *Westwd.*, as if it were repelled. This is the *reverse* of an iron action.

11952. The cask was now placed below, with its top level with the magnetic needle. The extreme distance was 62 inches and the near distance 1 inch. Now with side 2 towds. the needle there was no effect or only a trace to the left. Side 3 to the needle, no effect or a trace to the left. Side 1 nothing. Side 4 nothing.

11953. It would seem by this that the deflection when the cask was raised could not be due to the cask itself but to its contents. Also that it could not be due to the weight of the cask and contents depressing or changing the pavement, for that must have been nearly the same when the cask was below as when raised. But that it is not due to the sulphate of iron as a para magnetic action is evident, because a spike of iron in its place, when raised made the image in the telescope go to the *right*, and when lowered sent it to the left.

11954. Perhaps the flexure on the floor may have altered even the place of the telescope, but I cannot tell. The effect was very small

after all, and I do not accept it as evidence that the sulphate of iron had *any sensible action* of its own, either paramagnetic or diamagnetic.

11955. *Cast Iron.* Six bars of cast iron, making together a compound bar 4 feet long, by 2 inches wide and 1·75 inches thick, was held up in the area to the West of the magnetic needle, $13\frac{1}{2}$ feet off, and raised so that the lower end was $2\frac{1}{2}$ feet above the level of the needle. Whether held vertically or horizontally in the plane of the Magnetic meridian, they did not affect the needle. The Window was between and at the window there are 10 or 12 wrought iron vertical bars, which may have interfered.

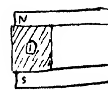
11956. The same bars 6 feet off to the East with no intervening iron affected the needle *properly*, but not much. At the distance of 3 feet the effect was much more. So the Earth's power strong and abundant, and not very easily disturbed by a small thing.

11957. An iron spike or anvil (wrought iron) about 24 inches long, 6 inches diameter at one end and 3 inches thick in the body, having a weight of 70 lb. or 80 lb. or more, when placed 3 feet off, affected the needle considerably and properly, i.e. as it ought to do, whether above or below or horizontal or with either end uppermost. But the effect was nothing like so great as I expected it to be, and considering what it did, the insensible effect of the sulphate of iron in the cask was no longer surprizing.

11958. The carts and carriages that go bye certainly affect the needle, but I doubt whether it is a magnetic effect, and not rather some effect of pressure on the ground and flexure of the whole building, and so change of place of the iron in it and consequently of the magnetic effect of the whole.

11959. As to the suppositious iron bar (11941). An iron wire having the diameter of 0·014 of an inch had 3 grains weight taken, which measured 14 inches. A piece $\cdot 014 \times 30 = \cdot 42$ of an inch and that would weigh 0·090 of a grain—a piece 0·21 in length and 0·007 in diameter would weigh only 0·01125 of a grain—and a piece 0·14 in length and 0·00466 in diameter would weigh only 0·00333 of a grain. Would such a piece as the last affect the needle at $\frac{1}{2}$ an inch distance? Or would a piece $\frac{2}{3}$ the length or 0·093 in length weighing only 0·00222 do so, and as much as the bar (11941) twenty feet off?

11960. *Magnetic needle in iron.* A small magnetic needle about 0·1 of an inch long suspended by cocoon silk was prepared. Also a block of iron (soft) about 2 inches square, with a hole about ½ inch in diameter and 1·25 deep in the middle entering in at one side. Also the long bar magnets. The long bar magnets were put edge up on the table as shewn*, being connected at the further end by soft iron; then the small magnetic needle place[d] at $|^{S}_{N}$ vibrated under intense power so that its vibrations could not be distinguished by sight. Then when the block of soft iron was placed between the magnet poles, and the same needle gradually introduced into the hole, whilst out of the hole it stood as before, but the force on it was very much less than when the block was away. When *in* the hole, the force was *still less*, *but* the direction of the force was the same in all cases, i.e. the needle at rest stood parallel to itself whether the iron was there or not (11683, 7).



11961. By changing the magnetic poles outside the iron, the needle within the iron changed its direction. There is just one flood of power, through the iron and the air in and around it.

11962. A piece of soft iron 6 inches long and 2 inches square with a hole in the middle gave a like result but in a weaker degree.

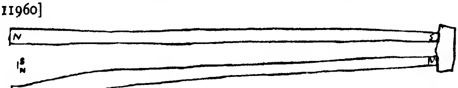
11963. If a bar of soft iron having a depression in it to receive a small needle magnet be demagnetized, and then a needle magnet be prepared which can lay in the notch, the iron filings over such a magnet away from the iron gives a very good set of curves. If the needle be put into the notch and then iron filings be sprinkled on paper over it, there are no signs of curves, or the very least, so thoroughly is the power conducted on by the soft iron. If thin paper be placed between the iron and the needle, then the filings over the needle produce weak curves; if the needle be raised a little from the iron then the curves come out more strongly.

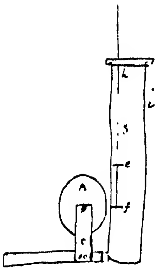
11964. Using a block of hard steel, and placing the needle upon it, the curves by filings come out more strongly than on the soft iron, i.e. hard steel is a worse conductor of the magnetic force than soft iron.

11965. Value of abundant conducting matter in terrestrial magneto electric inductions. A is a solid ball of cop[p]er 3·2 inches

[1] 1851 has been written here: evidently a slip.

* [11960]





in diameter, supported on copper axis by a wooden stand, C, so as to rotate by the hand; *e f*, an astatic needle pointing pretty well because not adjusted; it is supported by cocoon thread *g* to a wire *h* and enclosed in a jar brought close alongside of the ball. When the ball is revolved one way, the magnetic needles, which at first are parallel to its axis, then point vertically, so strong is the action. If the rotation be in the contrary direction, then the oppos. magnetic pole goes towd. the ball. The effect is very excellent.

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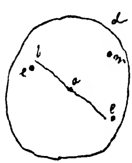


11966. Experimented on possible set of a wire when surrounded by an electrolyte and between two electrodes—*d* is a finger basin filled with water, *a b* a piece of copper wire bent thus* and about this length, suspended by cocoon silk from *a* and quite immersed in the water. Then wire electrodes from a battery of five pair of Grove's plates were put in the water in different places, as at *c* or *e*, or elsewhere. There were very little signs of motion in the wire in any case. When all was still, it kept its place whether the battery connexion was made or not.

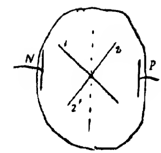
11967. Added some Sulphuric acid so as to make the water a conductor, and now the Neg. wire produced a stream of hydrogen gas and the Pos. wire a descending stream of sul. copper and also a little gas occasionally. When the wires were close to the moving wire, as above at *e* and *c*, and on opposite sides of the end of the slung wire, there was approximation, as if attraction; when the N. wire only was near one end, the Pos. wire being away as at *m*, then there was also apparent attraction. When the P. wire was near the end and the N. wire at *m*, then there was much less signs of approximation.

11968. I refer the apparent attraction at present to the mechanical effect of the evolved gas at the N. pole, which, causing a current to set upwards, draws in the neighbouring water and so the end of the horizontal wire with it.

11969. In order to get rid of this current, I removed the dilute Sulc. Acid and placed in the finger basin a saturated solution of Sul. copper. Now there was little or no gas evolved by the electric current—but the apparent attraction was also much less. There

* [11966]

were signs of it, but then there must have been *up* or *down* currents at the place of the electrodes from the electro chemical action, and either of these or both conspiring would tend to shew effects of approximation. When the electrodes were placed at a distance from the ends, I saw no sensible action.

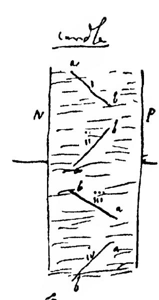


11970. Then I employed two copper plates as the electrodes, arranged on opposite sides of the basin and fixed carefully, and now there was an effect with the copper wire and the solution of sulphate of copper: for if the wire were in the position either of 1 or 2, or any corresponding position, it slowly moved into an *equatorial* position. The effect was small and the power small, but the result clear.

11971. As the wire was bent, this might be some action of a current of fluid formed at the wire which, in passing up or down from it, set the wire round as a vane. Still, it is remarkable that in either position it went into the equatorial position, moving to do this in *contrary* directions. Yet then the same end has contrary electrolytic actions in the contrary positions, and that may do something. To settle that, I must make the same end of the wire stand at 1 for instance and at 2′, for then any vane action which would move it from 1 to the equatorial position, going as the hands of a watch, ought to move it from 2′ into the axial position and so on, also like the hands of a watch.

11972. If the contrary happens, then there must be some peculiar setting force here. Perhaps analogous to that of diamagnetic action. Try nonconducting bodies in the Electric stream.

11973. There were present at these experiments to-day M. Dubois Raymond, Dr. Bence Jones and Mr. Barlow and also Mr. Anderson.



7 MAY 1852.

11974. Used the same apparatus, solution, plates, wire, etc. as before, also with 5 pr. of Grove's plates. To represent the positions of the wire and direction of the chief current, etc., the ends of the wire may be called *a* and *b*. So there may be four positions as in the figure. The observations were always made when looking across the liquid (i.e. along the slung wire) from below in the

diagram, the light when used being on the opposite side as at *candle*.

11975. When the wire was near the surface but fairly submerged —its motion was doubtful and uncertain. When about half an inch below the surface, it went freely from all four positions into the equatorial position—again and again. When placed lower, at the middle depth, it passed a little axially, i.e. feebly, but axially from all the positions. Being still lower—it went strongly axially from all positions. When placed near the bottom of the liquid and below the level of the lower edges of the Electrodes, it scarcely moved at all. Above and below the middle part was the best places for motion, the results then being contrary in the two cases.

11976. Looked for the currents in the fluid produced by chemical action at the two ends of the wire when oblique or axial by putting a light on one side the basin and observing with the eye on the other side, as above or in the figure. When the wire was equatorial, there was scarcely any currents or striæ—only a little refraction along the wire; but when oblique as in the figure or axial, then there were currents and striæ, easily and beautifully observed, and these were descending from the P. end of the wire and ascending from the N. end of the wire, whichever end of the wire by position happened to be in either of these states. These of course depend upon the solution and precipitation of copper, and do not occur when no current is sent through the solution.

11977. When the wire was about half an inch below the surface and the battery connexion then made, the descending current at the P. end of the wire at an angle of 45° with the Electric axis was watched. It began to descend perpendicularly but soon became a little deflected, the wire moving a little at the same time equatorially. But the bow in the striæ preceeded the place of the wire, even to the extent of $\frac{1}{3}$ of an inch or more. Yet the current of striæ was not broken in the fluid, as if it forced its way through it, but was clean and only little disturbed, and the fact seemed to be that the *fluid* below the wire appeared to move on and carry the current and *also the wire* with it. The successive forms are given above*.

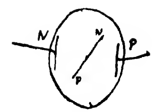
11978. This happened on both sides of the axial line, i.e. in the

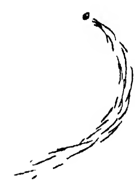
* [11977]

positions i and ii (11974): the descending currents being always from the P. end of the wire and the convexity always towards the equatorial line or plane.

11979. In this high position of the wire, the ascending currents and striæ from the N. end of the wire is by no means disturbed in this manner: it is very short and is disturbed only by the gradual change of place of the moving wire.

11980. Instead of the moving wire, used a similar copper wire equally long, straight, horizontal and capable of being *fixed* at any height or in any position. The currents and striæ were in relation to the P. and N. ends as before (11977). When the wire was a very little way under the surface, the descending currents from P. end tended very slightly to go to the equator or in that direction. When lower in the solution, about half an inch down, the currents stream off after a little while very much towards the equator—as much as before (11977). When about ⅔ of an [inch] down, there was the same effect. On first establishing the electric circuit, the descending current is straight and then gradually becomes more and more deflected, as in the figure*, so that the stream, which was first directly downwds., becomes curved and large as in the figure. Besides which, it changes its direction in different parts of its course, the course of the stream being ultimately first to the right hand, then gradually descending and at last going freely to the left. So that even when the wire is a fixture, the falling current descends in a curve, and if the parts beneath and near to the wire are carried right by a motion of the whole fluid, there must be a motion of the parts still lower to the left.





11981. The effect strikingly increases as the electric current is continued, as if masses were gradually put in motion. If it be that the mass of fluid below the wire tends to move and not the mere striæ current or altered solution, then the mass immediately below the wire must move well to the right hand, for the striæ current bows thus; and the parts below must move to the left, for the stream there goes left.

11982. If the wire were axial, there would be striæ streams but they would be vertical, for the convexity is produced towards the equator on either side of that position. If the wire were equatorial, there would be an end of the phenomenon also, for

* [11980]

then there are no striæ streams, there being no P. ends of the wire. So the effects of this descending stream or of the position of the wire producing it are confined to half a circle and divided in opposite directions into two quadrants.

11983. I slung a small piece of bibulous paper on a cocoon thread about 14 inches long, and then sunk it in the fluid; it could be put at any level by the thread, swinging like a pendulum slowly, but still able to shew horizontal motions in the mass of fluid. When a little below the wire, as at 1, 2, 3 or 4, it moved to the *right*, clearly shewing that the mass of fluid there moved and not the stream of striæ only: the latter simply indicate the general movements of the former. So that all the fluid near this part of the wire appears to be in motion. When placed at 5, just over the wire, I think it moved to the left–indicative of a tendency to rotation round the wire: but the wire was near the surface here and we shall probably see this better when the wire is mid way in depth, or lower down even.

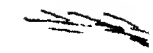
11984. Will an independant varnished and so insulated wire carrying a current of its own, cause like motions in the fluid?

11985. When the wire was deeper and about the middle of the fluid, the striæ stream did *not* go to the right hand at all, for though they gradually became curved, the effects were successively as in the figure, their course being to the left below. In no part of their journey was there any tendency to the right. This does not look so much like a tendency to rotation round the wire as like a certain relation of the middle part of the electrolyte (under the conditions) to the top and bottom parts.

11986. When placed lower in the fluid, the striæ stream still descended first vertically and then went to the *left* hand, as above. The ascending streams at the further and N. end of the wire could also be observed thrgh. the fluid at the same time, and they were found also to go to the left, according with the fact observed before, that the convexity of the descending stream is towards the equator of the fluid whilst that of the ascending stream is from it. The position of the eye and the inclined position of the wire being always kept in view.

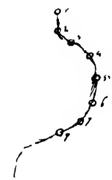
11987. The wire being lower still, the near descending stream went short of[f] to the left. The further ascending stream was then also enlarged in its curvature, as thus.

11988. When placed still lower, the descending stream was straight down, but the further ascending stream now went well to the apparent right first and to the left when higher up, giving the whole of the concave or dish shape result.

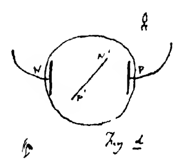


11989. So whether high or low, either the near descending stream or the distant ascending stream, or both together, describe the same apparent figure to the eye in the same place. It is of course clear that, in the complete curve from either end, the wire is below for the one curve and above for the other; so that if the motions of the fluid producing the curvatures be referred to current existing in the wire, the fluid around it appears to move in contrary directions, though whilst the eye keeps it[s] place the directions form one simple curve.

11990. Must make out very clearly why the motion in the same place as regards the wire (as for instance just below) changes with change of place in the wire; the wire if at 2 having fluid moving to the right beneath it, and if at 7, moving to the left, the electric current through the wire being always in the same direction. If the wire be at 1, the descending current is as in the figure, but if it be at 2, 3, 4, etc. etc. in the fluid, the whole curve does not descend with the wire, but seems stationary, as if it depended upon the mass of the fluid; only that part of the curve appearing which is below the wire in the figure. Of course the currents through the fluid (of electricity) will change with change in the place of the wire.

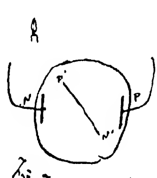


11991. Now examined the ascending striæ from the N. end of the wire, the eye and the light being generally on the same side of the glass basin and therefore on the same side of the stream of electricity through the electrolyte as before. In the former case matters were arranged as in figure *d*. Now they were adjusted as in fig. *a**. Before, the descending current was at P′ near the eye and the ascending current at N′ farther off. Now, the ascending current is near the eye and the descending current farther off. The equatorial plane is now to the left of the observer; before it was to the right.



11992. When the wire was near the top of the fluid, the ascending striæ went straight up. The descending current on the further side was first bowed near the middle of the liquid and afterwards

* [11991]



higher up, as if the parts about the center of the mass of fluid began to move first and not those parts nearest to the wire. The convexity of the descending current, being on the further side of the liquid, was *as always* towards the equator.

11993. When the wire was a little lower, the result with both the ascending and descending currents was the same.

11994. The wire a little lower, and now the ascending current went straight up, but the curvature of the descending current after a few moments was immediately from the wire, there being no previous vertical or perpendicular part. The perpendicular part was represented in some degree by the ascending portion of the rising current from the N. end of the wire.

11995. When the wire was a little lower, both the ascending and descending striæ currents were curved thus: both apparently in the same direction, but in reality the ascending current (near the eye) from the equatorial plane, and the descending current (on the further side) towards the equatorial plane.

11996. When lower and near the middle of the fluid, both the currents (of striæ) arch in the same apparent direction thus, and very equably making one simple curved or reversed C. Both the streams move off vertically from the wires; there is no break in the back of the conjoined curve as thus*—according in this respect with what was before observed (11987, 11994).

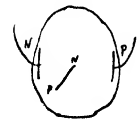
11997. When the wire was lower, the results were as here figured, the ascending stream beginning to have a first motion to the right and the descending stream now going off from the wire at once to the left.

11998. When placed very low, all the curvature was in the ascending current, as figured. It is curious to see how thoroughly the bow is confined to the middle layer of the solution: for now the ascending current is straight up or even curved a little irregularly to the left close to the wire, and takes its progress to the right higher up, returning to the left still higher up, and acquiring a reverse curvature in some degree again towards the top. This is absolutely the course of the striæ current through the fluid, so that if the fluid moves to the right in the middle of the depth or bow, it must also move to the left above.

11999. Made the wire only half the length, being now about

* [11996] 

$1\frac{1}{2}$ inches long. When placed in the middle of the fluid, like results to those obtained before were observed, but they were not so strong or so favourably shewn. When the wire was out of the middle, i.e. one end at the middle and the other near to one of the side electrodes, as thus, then the striæ currents appearing at the end in the center, whether that was N or P, were not much affected—very little indeed—but those at the sides as at P were more so and distinctly and as before.

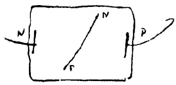


12000. These currents in the fluid are the cause of the *set* of the wire as first observed: I mean the currents or motions of the whole body of fluid or the part above the wire. The striæ currents are I believe only indicators of the former, and are not the causes of them—but the use of a separate insulated wire and current will develop all that.

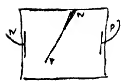
12001. Dismissed the wire and used another long one, about three inches in length, but well varnished so as to be insulated from the solution. When slung in the fluid, however, it was found not to be quite insulated, for there were small portions of striæ currents formed at both ends, and they obeying the laws before observed but feebly only. The wire, if it moved at all (in the positions of motion), did so only to a very small degree. So that it appears the effects when obtained are due to the electrical changes produced by the wire as a conductor immersed in the electrolyte, and not to any relation of the copper to the surrounding fluid, like that of diamagnetic bodies, or at all analogous thereto.

12002. A platina wire was used. Gas of course appeared at the positive end, and as the wire was suspended by cocoon silk and left mobile, this gas both disturbed the equilibrium so as to tilt the wire much and also disturbed and destroyed the descending striæ stream that appeared in the former case. Being however thus tilted and therefore inclining from top to bottom of the solution, it apprd. to go axially from all four positions (11974).

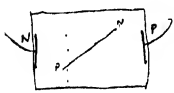
12003. A bar of heavy glass 2 inches long was suspended in the solution. Of course there were no striæ nor conduction through it. It appeared to be quite indifferent to the electric current across the electrolyte. The bar was heavy and sustnd. by ten cocoon threads, so that the torsion was considerable; but I believe there was truly no effect or tendency to any.



12004. Continued the same experiments. Used a square glass cell about 4·5 × 3·5 inches wide and 3 deep. The old Electrodes, $1\frac{1}{2}$ inches wide and as deep as the fluid or cell. A solution of sulphate of copper saturated and then diluted by one fourth its vol. of water–which liquid was $2\frac{1}{2}$ inches deep in the cell. Used a Grove's battery of five pair of plates. The direction of the current was as before through the fluid. The fixed copper wire was introduced and as soon as the battery connexion was made, there were the falling striæ streams as before from P*, gradually becoming curved–being convex towards the equatorial line or plane.



12005. Placed the wire in thus. The results were the same. The ascending current also on the further side was present and concave to the equatorial line as before.

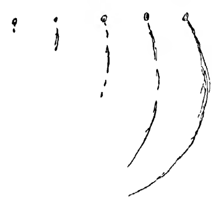


12006. The wire was placed in more obliquely–then looking along the wire the striæ convexity was not so great as when looking across the fluid in the direction of the dotted line. The curvature seems to be in planes nearly parallel to the sides of the trough or cell.

12007. Placed the electrodes in various parts of the electrolyte and the fixed wire in different directions across it, as in the follg. positions†. The effects were generally the same–but in the last case the flexure was not so great at the end of the wire touching the glass as at a part farther in to the mass.

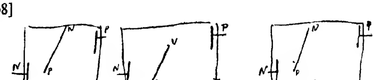
12008. Shortened the wire and then placed it as in the figures‡. In the first case the effect at the P. end was good. In the second case not so good. In the third case very good indeed.

* [12004]



† [12007]

‡ [12008]

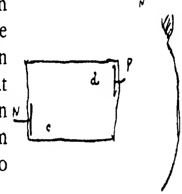


12009. Now keeping the last position, the wire was placed at different depths; both the ascending and descending striæ streams were as on the former occasion.

12010. I fixed a crystal of sulphate of copper by forcepes in the solution at the top–it dissolved and formed striæ–its place was over P, and when there was no electric current, the striæ descended perpendicularly on to P; but when the electric current was on, it went to the right of the wire first and then round it and to the left–shewing most clearly a motion of the whole fluid about the wire and not of that at P only or of the striæ only at P. No doubt the same or a like effect will occur at the other end. This was when wire was midway down. When somewhat lower, the same effect was produced. The ascending striæ at the other end of the wire were seen at the same time.

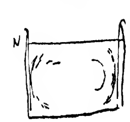
12011. When the wire was quite at the bottom, the descending striæ from the dissolving crystal shewed exactly the same effects. The ascending striæ on the further end N shewed the same large like curve.

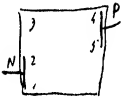
12012. As the motions appear to depend on the fluid more than the wire, I took out the latter altogether and employed only the crystal and its striæ as indications, and that in various parts. When the crystal was at *c* and no electric current, the striæ went straight down; but when the current was on, they gradually changed in direction and became curved exactly as if they proceeded from the P. end of the wire. So that the wire appears to be of no importance in producing the effect.



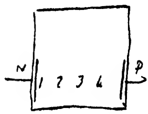
12013. Placed the crystal at *d* over the place from whence ascending currents in this apparent form would *rise*. The descending striæ gradually though slowly curved in the opposite direction, i.e. convex to the equator and chiefly above the middle depth, clearly shewing the motion of the fluid, and like the ascending current shewing that it is towards the equator above, and from it lower down.

12014. All this seems as if the motion was due to currents in the general mass of the fluid–and as a descending current must be formed against the P. Electrode and an ascending current against the N. Electrode, it is probably *these* set the fluid in motion, for they seem fit to produce all the effects. I now therefore placed

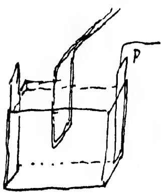




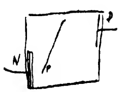
the crystal so as to give descending currents from the different localities marked. At 1 the striæ bend towards the equator. At 2 the same. At 3 little or nothing, at 4 and 5 doubtful.



12015. Again arranged things thus and the crystal at the figures. At 1 the deflection caused by the electric current was clear, *out* from the electrode above and *in* beneath, as if the fluid against the electrode were rising and so flowing off above and towards it below. At 2 the same thing. At 3 not sensible–at 4 not sensible–at 5 doubtful, but I believe convex to the P. electrode, as if it went in above and out beneath.



12016. Arranged an insulated wire in the fluid with a battery of 2 pr. Grove's plate[s]. There was no effect shewn by the crystal striæ near this wire, either when the current went through it or the other current went through the fluid, or when both currents passed at once. So no relation of this current to the fluid.



12017. As the effect with the P. end of the wire in the solution near to the N. electrode was very good, restored this state of things, but put the N. electrode into a porous cell with crystals and solution of sul. copper, so as to prevent currents in the fluid from the solution demetalized against the electrode. Now there were the striæ from the end of the wire both ascending and descending–*but no flexure* of them. Used a crystal by the P. end of the wire, but no deflexion of the striæ.

12018. All is due to currents in the mass of the fluid, due to the altered specific Gravity of the parts against the electrodes when the Electric current is on.

12019. A wire suspended in the electrolyte does not seem to set–but that fact is not decided in these experiments. The motions observed were not due to such an effect.

12020. For some time past have been getting the torsion balance into working order. Newman has put a frame under it so as to deepen the box, and has added a moveable chamber so as to allow a prolongation of the beam. I have described all the needful parts in MS., so may save time of description here.

12021. The *beam* is now a glass tube which passes through the box and attached reflectors. It is light; has the magnet end bent down about 2 inches, and terminated in a small well made glass hook. This end is to support the single object put under experiment at once, and the other end is to carry counterpoises to be adjusted by shifting along the beam.

12022. The *telescope* is that used before. When it and the object are placed at a given distance from the reflector, the degree may be estimated in size, for if diameter is 1, or radius, i.e. distance to the reflector, 0·5, then a degree by reflexion, being then doubled in extent, will be ·01745, i.e. very nearly $\frac{1}{30}$ of radius; so if the telescope and the object be 5 feet from the reflector, a degree will be 2 inches.

12023. *Scales* and objects to be seen by reflexion.

12024. *Guage* for adjustment of object and its suspension.

12025. *Stands* for retention of objects.

12026. *Screws* for adjustment of the box and balance on the table over the poles.

12027. *Battery* connexion and commutator at the telescope.

12028. *Gear* to put on torsion force whilst observing.

12029. *Guage* for height of torsion beam and for transverse equatorial line.

12030. Index for horizontal distance of the object from the magnetic core.

12031. *Temperature* bath.

12032. *Experimental cell.*

12033. Have prepared some objects for experiments and must keep a clear list of them all. The objects are of this nature: a clear rod of glass was taken and drawn out at one place into a filament—then the cylinder part cut off a certain length, as an inch—cleaned

at the cut scratch by sand paper and fuzed. The filament was also cut off and the end bent into a loop; the filament was set on by a guage concentric to the cylinder, and its length from the point of suspension to the center of the cylinder made just $4\frac{1}{4}$ inches. These are objects to hang on the glass beam and be submitted to the magnet. In other cases, as of bismuth, the suspension is a platina wire of such thickness that 5 inches weighs about 10 grains.

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12034. I mark the glass objects by diamond scratches up to 7 and then make a Vertical score for 8; so that

– = 1 ≡ = 4 ≣ = 7 | = 8 |‾ = 10 || = 16 ||||‾ = 34 etc.

It is needful that the mark should carry no iron and hold no dirt.

12035.

	No.								
	1. Flint Glass cylinder–	length	inches 1·5	diam.	0·254	weight	66	grains	suspension glass
	2. ”	”	1·5	”	0·272	”	78	”	Do.
	3. ”	”	1·5	”	0·33	”	115	”	Do.
	4. ”	”	1·5	”	0·385	”	161	”	Do.
	5. ”	”	1	”	0·245	”	48	”	Glass and platina
same rod of glass	6. ”	”	1	”	0·245	”	43	”	Glass alone
same rod of glass	7. ”	”	2	”	0·245	”	86	”	Do.
diameter 0·24	8.[1] bismuth short cylinder					bismuth	45	”	platina 15 grs.
Do. 0·315	9. Do.					bismuth	104	”	platina 8 grs.

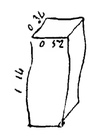
A piece of flint glass tube, in diameter and of such thickness that when stopped at the ends and full of air it just sunk in water, was used for the Six following tubes, i.e. 10, 11, 12, 13, 14, 15.

							whole weight	
from same piece of glass tube	10. length of cylinder		2	inches–	filled with	*distilled water*	158 gr.	glass film suspension
from same piece of glass tube	11. ”	”	$2\frac{1}{2}$	”	Do.	distilled water	196	Do.
from same piece of glass tube	12. ”	”	$1\frac{6}{8}$	”	”	*oxygen*, one Atmosphere	90	Do.
from same piece of glass tube	13. ”	”	2	”	”	*oxygen*, Do.	107	Do.
from same piece of glass tube	14. ”	”	2	”	”	oxygen, Do.	106	Do.
from same piece of glass tube	15. ”	”	$2\frac{1}{8}$	”	”	*Air vacuum*	108	Do.

16. Water
17. Alcohol common–S.G. ·842 at 68°.
18. Alcohol absolute–S.G. ·816.
19. Ether S.G. ·734.

[1] This line is struck out in MS., because object 8 was eventually destroyed (par. 12050).

20.
21.
22.
23.
24.
25.
26.
27.
28.
29. Cylinder common *lead*—225 gr.—repelled in air and water.
30. Common *Zinc* amalgamated—157 gr.—repelled in air and in water.
31. *Zinc* pure. Repelled in air and water. Weight 136 gr.
32. Cylinder of *Tin* common—weight 183 grains. Attrd. in air and water.
33. Antimony common—weighs 108 grains—a short square prism.
34. Sulphur—a crystal—48 grains.
35. Phosphorus—allotropic, Schroetter—weighs 38 grains.
36. Phosphorus common—weighs 41 grains.
37. Borate of lead fuzed—81 grains.
38. Rock crystal—58 grains.
39. Battery lead—considered pure lead—267 grains.
40. Zinc tree lead—considered also pure—253 grains.
41. Pure Zinc—115 grains.
42. Pure platina—22 grains.
43. Battery tin—pure tin—170 grains.
44. Heavy Glass—292 grains—1·14 of inch by 0·52 and 0·34.

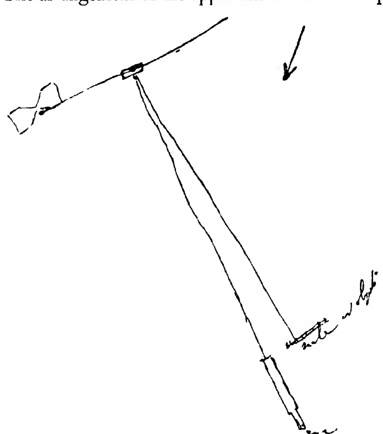


31 JULY 1852.

12036. The balance is advancing towards its completion. The beam is suspended by the former torsion wire (11108) platina. Its vibrations are exceedingly slow, indicating the small amount of torsion force. Shall see whether it is too delicate.

12037. The arrangement of the apparatus is thus* as respects the

* [12037]



magnetic axis—the *object*—the *reflector*—the *observer*—and the *scale* observed. So when to the naked eye the image in the reflector appears to go to the *left*, the object is *receeding* from the magnetic axis or approaching the observer. When the image in the telescope appears to go to the right, then the same effect occurs, because the telescope reverses the appearance.

12038. By the use of several lines marked upon the table of the magnet and of other lines marked on the balance case and also on the magnetic poles—and by the use of the six setting screws outside the balance box—it is easy to adjust the beam and object very accurately to the magnetic axes. There are also several scales in various parts to assist and to observe the motion of the beam by.

12039. The objects appear to answer very well as to adjustment, etc. in the air.

12040. Tried an experimental cell and water to see how the oscillations went on. Found a narrow tube cell would not do, for the capillary attraction at the surface between the water round the suspending filament and the side of the vessel drew them together, and was enough to hold the object against 360° of torsion and more.

12041. Employed a glass jar 2·5 inches in diameter, with water, to see how far the effect extended. When the filament intersected the water surface at the middle, then it stood there undisturbed as it had done in air, i.e. at the same place or zero of the torsion scale. The last vibrations were exceeding slow here by the torsion force alone.

12042. Moved the glass in the equatorial line so that the place of rest should bring the intersection about 0·9 from one side and 1·6 from the other side. Here the object (a glass cylinder) took its right place according to torsion, not tending sensibly to go to the nearer side. Moved it again, so that the distances were about 0·8 and 1·7, or perhaps 0·7 and 1·8. The object was almost unaffected at the 0·8 or rather 0·7 distance, but on allowing much time, it was found to move very slowly and then gradually to be accelerated and to approach and adhere to the nearer side.

12043. So a less distance than 0·8 cannot be allowed at the surface. The glass suspension is somewhat thick—perhaps the $\frac{1}{30}$ of an inch, but that is desirable for *strength* and *stiffness*.

12044. The top of the experimental cell had better be a little trough, about 2 inches by 3 inches in size and half an inch deep.

2 AUG. 1852.

12045. Began Experimental trials, and first with Object 9 or larger bismuth cylinder (12035). When torsion 0°, its place if indifferent would have been about $\frac{1}{2}$ inch from the Magnetic axis. When hung on the beam, it was at that distance slightly repelled, the magnet being in its residual state and not having been employed for months. On adding 10° of torsion it approached a little nearer, but was clearly and well repelled; 20°, 30°, 40° of torsion were given and still the bismuth was so much repelled as not to come into contact with the magnet. With 40° torsion, its distance might be 0·1 or 0·08 in the equatorial line. With 50° torsion it did not touch it.

12046. When the telescope and its object were put on, the small vibrations of the beam were well observed and very conveniently. The vibrations are now *much quicker*, under the force of torsion and a quickly changing field, than when mere torsion about 0° acts on the beam—and so far much facility is gained in determining the place of rest of the beam and object.

12047. To see whether the object is separate from or touches the magnetic axis, it is well to have a small mirror beneath by which its reflected image can be seen projected on the luminous ceiling and in various positions.

12048. Excited the magnet by 5 pr. of Grove's plates, and now the bismuth went off powerfully, being repelled; it went off more than 1·5 inches with more than 100° of torsion, resting in fact against the side of the box (at the outer end of the beam). The object is too powerful and the effect too strong for this suspension wire. But it shews we shall have a good range of power.

12049. Tried the action of this object when the battery was disconnected and the magnet again residual; it was much stronger than before, bearing 90° of torsion at the distance of about 0·1.

12050. *Object* 8 or smaller bismuth (12035) being employed, was attracted by the magnet both in the residual and excited state. On breaking away the bismuth, I found the piece of platina wire

I had used for the suspension was paramagnetic and had caused the effect. This Object is now destroyed.

12051. *Object* 6, or small glass cylinder (12035) was employed. When the torsion was 0° and the magnet residual, it was very slightly repelled. When the 5 pr. of plates were connected, it was driven off and held away $1\frac{1}{2}$ inches, the outer end of the beam touching the box.

12052. Have arranged a *check or curb* at the outer end of the beam, being a fork of platina wire which limits the vibration of the beam either on the one side or the other and within any given space. Shall make this adjustible by hand. It is an important adjunct in experiments of observation.

12053. Kept the object 6 by the Curb a full inch from the axis of power. With the 5 pr. of plates it was instantly driven further off. Gave 180° of torsion, urging the object to the magnet—then the 5 pr. did not drive it further off—with 120° torsion and 140° it receeded. When the torsion was 160° and the 5 pr. plates connected, the object was slowly moved away and held just clear of the wire against which the torsion before pressed it.

12054. So with this cylinder of glass in air, at the distance of an inch and with Magnetic power due to 5 pr. of plates, the diamagnetic force is equal to 160° of torsion, with this size and length of platina torsion wire.

12055. The torsion wire is 24 inches long and could, I believe, have born[e] 2 revolutions without set—certainly not three (11108).

12056. When this Object 6 was 0·5 of inch from the magnet and with 360° torsion, the 5 pr. of plates drove it slowly away. At 0·76 of distance with 360° torsion, the same power just set it free. So plenty of power here.

12057. *Oxygen.* Employed the Object No. 12 (12035), being oxygen in a thick glass tube. As a whole, it is diamagnetic, which is what I wanted. I hope it will be so in water as well as in air. When 1·0 inch from the axis and with 40° of torsion, the 5 pr. of plates sent it still further away.

Worked further at the ballance.

12058. Beam. The length of the object arm to the point of suspension, i.e. the radius of revolution of the object, is nearly 5·25 inches. The radius of the torsion index and scale is 1·75 or $\frac{1}{3}$ of the former, so that a degree is three times as large at the object as at the scale of torsion.

12059. On the opposite arm of the beam I have also marked off the length of 5·25 inches. The arm is perhaps an inch and a half longer. Have placed a scale of inches divided into tenths under this arm, and by a shadow or by observation can watch the motions well here or tell where the object is.

12060. *Object suspension.* When the beam is brought into the horizontal position by the adjustment of counterpoise on the outer arm, and that is ascertained by the use of the horizontal fork support—as a guage—then the hook at the end of the arm is at such height as to require a length of 4·375 or 4·37 inches for the suspending filament between the center of the object and the under side of its hook. This brings the center of the object into the same horizontal plane as that including the magnetic axis.

12061*. A loop of this kind, made of platina wire fastened on to a cedar handle, is excellent for placing and removing the objects from the balance beam.

12062. Lines are marked on the table and box. After the table and magnet are fixed together by little wedges, a line is drawn across the table between the poles, parallel to the place of the future magnetic axis and perpendicularly beneath it. A second line at right angles to this is also drawn across the table, passing exactly under the middle of the magnetic axis. The intersection of these lines is to be under the middle of the magnetic axis. Lines are drawn also along both the double cone and the two side pieces of soft iron—which being made to coincide with each other and with the first line described, give quickly the place of the cones and therefore of the magnetic axis. The double cone is well supported on a stand of its own, and whilst adjusting its place, a little bob is employed to ascertain that it has its central part precisely over the intersection of the table lines. Thus its place is secured.

12063. The balance box has two perpendicular lines drawn down

* [12061]

the sides, one at each end, in the same vertical plane as the torsion wire suspension. At the lower end of each, a scale of inches divided is applied, so that when this vertical plane passes also through the first line ruled on the table and therefore also through the magnetic axis and truly through its center, the zero of the scales shall coincide with the table lines. Then it is easy by the adjusting screws to set this box either by this line or to any given amount on one side of it, which of course carries the center of suspension of the torsion beam the same amount to one side of the magnetic axis.

12064. Lines are also drawn on the other two sides of the box in that vertical plane which is transverse to the former and coincides with the center of the object when freely suspended from the horizontal beam. So when the box is set by the end screws, so as to cause these lines to coincide with the transverse lines on the table, then the object is truly in the equatorial plane of the Magnetic field or cones. These are important points gained.

12065. The beam was weighted by counterpoises only, so as to be free from the residual force of the magnet; and then the torsion plug or cone adjusted until the beam, when quite still, was parallel to the magnetic axis. Then the ring scale was shifted until the graduation Zero coincided with the index, and now that position is to be preserved as respects the scale.

12066. In this case the conical end of the beam or counterpoise arm was at Zero or a marked line of the scale in the box, and as this scale moved with the beam, it could always be used to observe the motion or position of the beam and its parallelism with the magnetic axis.

12067. The telescope was at the same time set upon the reflector and a graduated scale adjusted as an object. The inch marked 6 was made to indicate the normal position of the beam. Either the counterpoise end or the telescope could be used to watch the motions of the beam by and to preserve a record of the normal position. The telescope was by far the most delicate.

12068. A bridle or check fork is applied on the box at the counterpoise end. By including the beam there it limits its oscillations to 15° or 20°, and the limb of the fork can be used to hold the

beam in a given place when torsion is at the same time applied to it (12088, 90).

12069. In estimating the distance of the object from the magnetic axis when the latter and the beam are parallel, it will be best I think to count from the center of the magnetic cones along the equatorial line to the center of the object. Then this distance can be obtained at once by the use of the setting screw on the table and the end scales of the box (12038), and will be sure to be accurate.

12070. The *Experimental cell* has had a little trough of copper ½ an inch deep and 3 inches by 2 inches wide added to the top, and it now does very well indeed.

12071. Adjusted the poles and the beam box so that the beam was ½ an inch on one side of the vertical plane passing through the magnetic axis (   ). Hung up Object 6, being a small flint glass cylinder (12035). The residual magnetism affected it and repelled it, and it required a torsion of 4° to restore the cylinder to the given normal distance of 0·5 of an inch, *air* being the surrounding medium.

12072. Removed the object—put the experimental cell in its place and restored the object No. 6, and readjusted the beam to horizontality. Now the former torsion of 4° carried the object into or towds. the magnetic axis until it touched the cell inside. Even with this weak magnetic force, it shews beautifully the *relative effect* of the Glass, water and air, and also give[s] a sort of measure of the magnetic character of the air. I took off the torsion and even then the glass did not move from the side—seemed to be a little less diamagnetic than water.

12073. When filling the cell with water, must take care there are no bubbles of air sticking down below—they will interfere with the free motion of the glass cylinder.

12074. Put on the force of 5 Pr. Grove's plates. Then the Glass cylinder slowly receeded as if more diamagnetic than water, but nothing like as in air—no comparison.

12075. Left this experimental cell with the water in it exposed to the air—for 3 hours—then put it into its place and introduced the *Object* 6 (the same as before)—and now found it was very sluggish and did not move—as if it were in *mud*—or as if a holding

film had formed on the surface. Stirred the surface, but still the effect was the same. When moved to a new place it kept its new place.

12076. Put on torsion; 11° moved it a very little, then it stuck. Added more and more torsion, but not until 62° did it move of itself a little, and then it stopped again. With 118° of torsion it very slowly cut its way through the film. From this, brought the torsion back suddenly to 0°; the object remained immoveable in its last place. Gave 60° reverse torsion, but it was just as stiff and sluggish as before.

12077. Must be aware of this *film* on standing water and perhaps on other liquids. Very interesting. Washed out the cell and employed fresh distilled water, and now the object moved freely as at first, and when the torsion was 0° rested at the given distance of 0·5 of inch from the magnetic axis. Even from the first, however, the movements seemed more sluggish than in some other liquids as Alcohol or Ether.

12078. Brought the curb into use (12068). Put on torsion towards the magnetic axis and then adjusted the curb so that the beam, being held against it by the torsion, the counterpoise end of the beam and also the position of the object in the telescope (12067) shewed that the object was at the given distance of 0·5 of inch. Put on the force of the 5 pr. of plates, and then the object was repelled and set free from the curb. On taking off the current, it returned against the curb to its first position. Increased the torsion—at 90° the 5 pair of plates were just able to separate the object and beam from the curb. With 100° torsion I could not freely separate it; it shook and if a carriage went by, by tremor and elasticity went off and returned again, but did not continue just separate. With 80° of torsion it separated freely and in the telescope much, even up to a full inch of its scale. When the battery was off, it swung back with force against the curb.

12079. Here the telescope is very valuable. The observer can see the effect at the moment that he makes contact with the battery—he can tell when the beam is in the smallest degree free from the curb, and when the liberation would be absolutely insensible at the counterpoise end of the beam or at any other place.

12080. The water was removed. *Common Alcohol* of S.G. ·842

at 62° put in the Experimental cell. The same object No. 6 placed on the beam at the same distance as before and the experiment repeated. The torsion was raised up to 216° before it was able to make the glass cylinder keep its place in the fluid when the 5 pr. of plates were used. So that Alcohol is much less diamagnetic than water.

12081. As to film, by trial on both sides of Zero, there seemed little or no effect of that kind here.

12082. The same expt. with so-called *Absolute Alcohol*, S.G. ·816 at 62° F. The torsion needed was 250°.

12083. Then with *Ether* in the Exp. cell. The torsion now was 345°. This ether by evaporation would be at a lower temperature than the former liquids. I do not know as yet whether that produces any effect. I did not find that any currents due to this cooling at the surface interfered with the determination of the torsion force. Ether, Spec. Gravity ·734 at 62° F.

12084. Water again—fresh—to ascertain whether battery power or power of the magnet had changed. It gave 92° torsion which is beautifully close.

12085. So these four fluids gave the following numbers:

temperature generally 69° F.	Water	91°	S.G.	1·000
	Alcohol	216°	S.G.	·842
	Stronger Alcohol	250°	„	·816
	Ether	345°	„	·734

12086. I now altered the distance of the object center and the magnetic axis from 0·5 to 0·8 of an inch, and repeated the experiments with the same object and fluids as before. The torsion required was of course much less, being as follows:

Water	35°
Alcohol	63°
Stronger Alcohol	76°
Ether	113°

12087. These numbers are in the same order, but not in the same ratio, and cannot therefore express directly strength or differences of strength. If a series be constructed for the first expts., having 91 for water but with the same proportions as that for the last results, it will give the results in the second column and not those in the third: yet the difference from the third column is

	Last series		First Series	
Water	35°	91	91°	
Alcohol	63°	164	216°	bad results (12088)
Stronger Alcohol	76°	198	250°	
Ether	113°	294	345°	

remarkable and regular, being just 52° in all the cases except water, when it is of course 0°.

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12088. The whole day almost in vain, for at the end of it I discovered a source of error which vitiated all the results and also those of yesterday—but it was well to know the error. The curb (12068) which I used to confine the vibrations of the beam was of platina wire, the beam itself of glass, and these two when pressed together by torsion adhered with such force as not to allow the separation or displacement of the object when the magnetic force even surpassed the torsion force. In one case the adhesion rose up and equalled 310°. No wonder the results yesterday were incomprehensible. Those of to-day I shall not enter except as regards any useful points.

12089. The order of the liquids and glass is as follows, and also including air:

Air	Paramagnetic	
Ether	S.G. ·734	
Alcohol Absolute	S.G. ·816 or 0·0 by Sykes	69° F.
Alcohol Comm.	S.G. ·842 or 15·2 „ „	
Water		
Flint Glass	diamagnetic	

12090. Used a card curb or bridle (12068) and now the beam obeyed the torsion force and magnetic force beautifully, shewing well when they were equal and when the beam was separating. Nothing left here to be desired.

12091. When the Exp. cell contained *water* and Object No. 6 was in it, distance from magnetic axis of 0·5 of inch, then the ten pair of plates at the end of the day caused separation with 125° of torsion—half an hour after, exactly the same thing.

12092. Found the torsion this morning the same as yesterday,

in that respect very satisfactory. Made it quite correct, i.e. made the counterpoise end of beam correspond to ⊤ of the box scale at the same time that the object in telescope was 6 on the scale and the torsion scale 0° coincided with the index, the beam being parallel to the magnetic axis. 0·1 of inch in the beam end box corresponds very nearly to 2·0 inch in the telescope scale, for the distance of the telescope object is 67 inches and the length of the beam arm 6·5 inches—and then the doubling of the scale by movg. reflector.

12093. The torsion plug appears to be not quite central, for as it revolves, the image in the telescope coinciding with the wire is sometimes 5·8 and at others 6·2, and this difference depends upon the position of the plug and the place of the pointer, for it indicate[s] the same variation at 90° as at 450°, and at 180° as at 540°.

12094. The battery contacts ought to be by soldered ends and cups of mercury.

12095. Ten pair of plates does far more (perhaps double) of what 5 pr. do.

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12096. The *fork* is now right (12090); no adhesion there. The *plug* is not central and disturbs the telescope object a little in its revolutions (12124). Torsion does not do this sensibly. When the beam had torsion on it of 50°, the scale division was observed in respect of the wire through the telescope. A revolution of torsion was then added, or 360°; this did not displace the mark in relation to the wire. A second 360° of torsion were added and still the place of the wire on the scale was the same. So all right here.

12097. Charged 10 pr. of plates, Grove's, and placed 5 of them in connexion with the battery wires. Employed Object No. 6—the glass cylinder (12035)—in *Air* as the surrounding medium, with an interval at Magnetic axis and object of 0·5 of inch (12069) between center and center. It separated well with 5 pr. of plates. The torsion force requisite to meet the separation was 1060°.

12098. Made the distance 0·7 of inch—then the torsion force 440°. After standing ten minutes, the same amount of torsion force was given, as if the battery and Magnet pretty constant.

12099. Then went through the following experiments in the order given, using always the same 5 pr. of plates. The fluids were of the following character and S.G.:

Water	S.G. 1000	64° F.
Common Alcohol	842	64° F.
Absolute Alcohol	816	64° F.
Ether	734	62° F.

12100.

Object No. 6	Interval of object and axis	Surrounding medium	Voltaic Batty.	Torsion force to balance
Glass cylinder	0·5 of inch	Air	5 pr.	1060°
”	0·7 ”	”	”	440°
”	0·7 ”	Water	”	51° temp. 65°.
”	0·5 ”	”	”	101° ”
”	0·5 ”	Common Alcohol	”	267°
”	0·7 ”	”	”	107°
”	0·7 ”	Absolute Alcohol	”	115°
”	0·5 ”	”	”	340°
”	0·5 ”	Ether	”	388° temp. falling
”	0·7 ”	”	”	171° from evapora-
”	0·7 ”	”	”	147° tion and at the
”	0·5 ”	”	”	376° end 56°.
”	0·5 ”	Absolute Alcohol	”	302°
”	0·7 ”	”	”	110°
”	0·7 ”	Common Alcohol	”	101°
”	0·5 ”	”	”	260°
”	0·5 ”	Water	”	106°
”	0·7 ”	”	”	49°
”	0·7 ”	Air	”	425°
”	0·5 ”	”	”	1100°

These experiments occupied 3 hours, and comparing the results with Air at the beginning and ending of the series—the magnetic force appears to have been very constant for that time.

12101. When like results from different parts of the list are compared, they are really very near to each other—those of Absolute Alcohol at 0·5 distance and Ether at 0·7 distance are the farthest apart. As yet also I am only learning to observe how the separation from the curb takes place, etc. The mean of all the results is as follows:

12102.

			At distance of 0·5 of inch		At distance of 0·7 of inch		
The same Glass cylinder with same 5 pr. Grove's plates	Air		1080°	0[1]	432°	0[1]	2·5
	Ether S.G. 734	2·556[1]	382°	698	159°	273	2·4
	Absolute Alcohol S.G. 816	2·375	321°	759	112°·5	319°·5	2·8
	Common Alcohol S.G. 842	2·49	263°·5	816·5	104°	328	2·5
	Water	2·55	103°·5	976·5	50°	382	2·07

See (12173).

12103. If we divide the 0·5 results by the 0·7 results, we get the third row of figures, shewing the ratios of the results with the same substance in the same media at different distances—and these approximate so much as to indicate that the effects are always proportionate when the distance is the same, i.e. when the [illegible] of the medium displaced is the same. In that case, more careful experiments would give a series of numbers for any given distance, which would have the same ratio whatever the distance chosen, and these numbers would directly express the magnetic force of the bodies. Thus making air 1000, the other bodies would be as follows—

	by the 0·5 results	or thus by the 0·7 results.
Air	1000	1000
Ether	372	368
Absolute Alcohol .	297·2	260·4
Common Alco[h]ol .	244	240·7
Water	95·8	115·7

Further experiments will probably make these numbers coincide. (See 12173, 12175).

12104. They are the *numerical expressions* of the magnetic force of the bodies—and should be confirmed by the use of *other objects*—and the employment of *other* degrees of magnetic force—and the results of *attraction* of the *objects* as well as repulsion.

See onwds. and (12160, etc.)

12105. Added on the other 5 pr. of plates—so that 10 pr. used—5 of which had not been used and 5 had been in use three hours or more. The distance was now made 0·7 to compensate for the increase of magnetic force. Object No. 6 as before (12100).

[1] These columns of figures are in pencil. See par. 12168.

In Air	755°	
Water	77°	
Alcohol Ordny. .	175°	
Alcohol Absolute .	197°	
Ether	225°	a little irregularity here as if
Ether	225°	departure of the beam from the
Alcohol Absolute .	195°	rest was a little uncertain.
Alcohol Ordny. .	172°	
Water	80°	
Air	770°	

12106. Can see here first that the effects are far greater than with five pr. of plates and the magnetism evidently greater. So we have command of the instrument in that respect to a large extent.
12107. Can see also that the distant (in time) observations of the same fluid are near to each other—much more regular than before (12100), for I am learning how to observe.
12108. When the mean is taken, the list stands as follows:

			which with air 1000 is	with 5 pr. of plates at the same distance the mean results were		
Object *No.* 6 with 10 pr. of plates and a distance of 0·7 of inch	Air	762·5	1000	432	1·76[1]	p. 2480[1]
	Ether . . .	225	295	159	1·41	
	Alcohol Absolute	196	257	112·5	1·74	
	Alcohol Common	173·5	227·5	104	1·67	
	Water . . .	78·5	103	50	1·57	

12109. Now the numbers with air at 1000 came either near to or between those with the 5 pr. of plates (12102), except with Ether, which before agreed very well, but here is a large way of[f] 70° or 80°. But it was just at Ether above (12105) that something irregular was remarked, and Ether coming in the middle of the experiments had not the advantage of distant observations.
12110. Therefore I think the results with 10 pr. of plates confirm the general conclusion as to principle drawn from those with 5 pr.
12111. Now intended to take a new object and the same media. Took No. 4 (12035), a flint glass cylinder like No. 6 but nearly 4 times as heavy and large. When on the beam in air and counterpoised, it stretched the suspension wire by straightening it, so

[1] The numbers in the last column are in pencil; the reference is to par. 12116.

that the telescope scale had to be lowered somewhat to come into view in the reflector.

12112. With a distance of 0·7 and ten pair of plates, the effect in air was too great for me to trust the wire with the full torsion required to counteract it. So made the distance *one inch* and then the 10 pr. of plates gave force in air as 540° torsion.

12113. Substituted water for Air; but found the glass paramagnetic in relation to the *water*. So it was also in the Common Alcohol—in the Absolute alkohol—and even in the ether. In the ether it appeared nearly balanced, but when by attraction it came within smaller distances than at first, the paramagnetic character of the glass above the ether was very evident.

12114. *Object No.* 3. (12035), being a smaller glass cylinder than the last, apprd. to be nearly indifferent in Ether—perhaps attracted. *Object No.* 2, slightly diamagnetic in ether. Object No. 1, attracted in Ether. No. 5, attracted in Ether. No. 7, repelled in Ether. No. 7 is the same glass as No. 6.

12115. Nos. 6 and 7 are two cylinders from the same rod of glass (12035), the first being 1 *inch* long and the second 2 *inches*, to see the effect of variation of mass, all other things being the same. These were placed in ether at the distance of 0·7 of inch and compared, 10 pr. of plates being employed. The first gave torsion 165° and the second torsion 322°, or very nearly indeed the double. This is remarkable considering the distance of the ends of the longer one: but must increase the expts.

12116. No. 6 was then compared in the same fluid, *Ether*, at the same distance of 0·5, with 5 and with 10 pr. of plates—the results were:

for	5 pr. plates	270°
	10 ,, ,,	430°

and dividing 430° by 270° it gives the ratio of 1 : 1·6. If we do the same thing with the results obtd. with 5 and 10 pr. of plates at a given distance, namely 0·7, in *different fluids* (12105), we have the following ratios:

Air	1 : 1·76
Ether	1 : 1·41
Alcohol absolute .	1 : 1·74
Alcohol common .	1 : 1·67
Water	1 : 1·57

12117. Now with the exception of *ether*, about which there is some, and much, doubt as to the results, all these accord so nearly as first results with the present as to indicate that the same general expression of magnetic force is obtained for the *different bodies* employed, whatever the amount of magnetic force employed, provided it remain unchanged for the time.

12118. Have now been working from 9 o'clk. until 3 o'clk. or later, perhaps 4 o'clk.—and therefore for purpose of comparison with the first results, put on the 5 pr. of plates first used with Object No. 6 in Air and ascertd. the torsion force for two distances:

at distance 0·5 it was 765°
0·7 „ 362°

in the morning it was (12102) 1080°, 432°. So these 5 pr. of plates appear to make manifest their exhaustion by the day's work. Still, the ratios for the two distances are not far apart, though the strength of the battery is so much changed: for in the early morning with strong battery it is as 1 : 2·5, and in the end of the day's work as 1 : 2·2 nearly. So that confidence in the apparatus and principles increases.

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12119. I have supplied the battery connexions with soldered and amalgamated ends and mercury cups, to make all secure and certain at the making and breaking of contact.

12120. The torsion wire has been left all night with no weight on it, but a torsion of 280°. In order to ascertain whether this or yesterday's work had given any set to the wire, the beam was balanced and the torsion made 0°. When at rest it was *not* parallel to the magnetic axis: the torsion index had to be advanced 7° to place it thus parallel, as if a set to that amount had taken place.

12121. As the balance box was at this moment 0·7 from the zero position, and therefore the copper counterpoise on one side of the vertical plane through the magnetic axis, the position was made 0. The result was that the torsion was still 7° or 8° out as before. Perhaps due to loss yesterday. Perhaps some other cause, as electricity of the glass, may be concerned.

12122. Made a fresh departure to-day by adjusting the torsion

circle scale to zero, when the counterpoise end was at † and mark 6 was in the telescope.

12123. Examined the position of the double cone on the magnet table; it has kept its place well—no source of error there.

12124. Examined the effect of excentricity of the torsion wire suspension ( ). The beam was without any object, but lightly loaded and balanced. A torsion of 40°, when bearing against the rein or bridle in the end box, the telescope gave the line 6 on the wire. Torsion was then increased up to 2½ revolutions and the coincidence of the scale with the telescope wire observed and recorded. The results are in the next page in black ink[1].

40°	6	*6·18*	1 Revol. and 20°	5·85	*6·24*
60°	6·1		40°	5·96	
80°	6·15		60°	6·06	
100°	6·2	*5·92*	80°	6·15	
120°	6·2		100°	6·2	*5·75*
140°	6·2		120°	6·2	
160°	6·15		140°	6·19	
180°	6·1		160°	6·14	
200°	6·04	*6·04*	180°	6·07	
220°	5·92		220°	5·91	
240°	5·85		260°	5·74	
260°	5·76		300°	5·64	
280°	5·72		340°	5·66	
300°	5·66	*6·5*	2 Revol. and 20°	5·84	
320°	5·68		60°	6·04	
340°	5·7		100°	6·18	
360°	5·78		120°	6·18	

So the extreme displacements occur in the same places or positions of the torsion head whether the torsion is little or much, namely at 120° and 300°—the limits being 6·2 and 5·64. Whenever the head comes round to the same position, the displacement of the object seen in the reflector is the same.

12125. After this, took off the torsion altogether and left the beam to settle. It was not then absolutely at Zero of position but seemed to have gained a very little set; but this gain was in the reverse direction to that which the recent torsion would have produced.

[1] See below. Figures in italics are in red in the MS. (see par. 12128); the remainder are in black ink.

12126. Put much weight on to the beam, changing nothing else; left the torsion index at 0°. There was still the same little difference as before—a mere trace. It is just possible that electricity of the glass may do this. Or the diamagnetism of the beam and its load, though it be so far above and over the magnet.

12127. The excentricity of the wire will act thus. There is a stop at the counterpoise end of the beam, against which the torsion presses it, and this becomes a center. Because of the excentricity, the wire at its suspension describes a small circle as the plug revolves; therefore the beam does not remain parallel to itself but describes an arc, and the end carrying the object move[s] to and from the magnet through a space which is about double the diameter of the circle of excentricity. This causes a variation in the distance of the object which ought not to take place, for in fact when in media differing in degree, the measurement is taken at times when the object is not at the same standard distance from the magnetic axis.

12128. To correct this, I applied a paper stop at the object end of the beam, attaching it to the under surface of the glass covering plate, and then took a few observations at those degrees which gave the greatest differences—they are entered in red ink (12124). From these it will be seen that differences of nearly the same amount as before occurred, which ought to be the case, because the stop is as far off from the center as before—but the *direction is changed*. The alteration is now caused by the counterpoise end swinging and *not the object end*; so that great good is gained by the arrangement, for the distance of the object from the magnet approaches far nearer to *invariability* than before.

12129. *Platina wire.* A coil obtained from Mr Newman for suspensions of objects had 6 inches cut off and hung up as a suspension and object. It was very strongly attracted by the magnet and 5 pr. of plates, and was even attracted by the magnet in its residual state. It will not do. I ought to find a glass rod having magnetic force nearly as air, and then draw that into filaments and use it for suspensions.

12130. In order to have some idea of the objects on the list (12035) and their relation to the media already employed, I tried several of them in *Air*, *Ether* and *Water*—to ascertain generally

whether they were attracted or repelled. The distance was 0·5 of inch and the power given by 5 pr. of Grove's plates—but these points were not important as the results would have been the same for other distances and forces.

			in Air	in Ether	in Water
	Object 1.	Glass cylinder small	Repelled	Attracted	Attracted
	2.	Do. . . larger	R	Repelled	Repelled
	3.	Do. . . larger still . .	R	R	Attracted
	4.	Do. . . largest . . .	R	Attracted	A
	5.	Do. . . small on platinum	R	Repelled	Repelled
same Glass	6.	Do. . . 1 inch . . .	R	R	R
same Glass	7.	Do. . . 2 inches . . .	R	R	R
same piece of glass tube	10.	Glass cylinder filled with distilled water . . .	R	R	Attracted
same piece of glass tube	11.	Do. . . Do.	R	R	A
same piece of glass tube	12.	Do. . . filled with *oxygen*	R	Attracted	A
same piece of glass tube	13.	Do. . . filled with *oxygen*	R	A	A
same piece of glass tube	14.	Do. . . filled with *oxygen*	R	A	A
same piece of glass tube	15.	Do. . . air *vacuum* . .	R	A	A

12131. So of these glass cylinder objects, Nos. 6 and 7 are the most diamagnetic, being repelled in all these media, and Nos. 2 and 5 are also always repelled and I believe almost as strongly. No. 3 comes next, being repelled in air and ether but attracted in water—whilst 1 and 4 are the least diamagnetic, being repelled only in air. They probably approach near to space in magnetic characters.

12132. As to the glass of the tubes containing oxygen, etc., it is evidently on the paramagnetic side of water, for Nos. 10 and 11 are attracted having water inside and outside. That 12, 13 and 14 should be more attracted and approach even in ether is because they contain oxygen, a paramagnetic body. Water is evidently on the diamagnetic side of space, because 10 and 11 are repelled in Ether, whilst 15 or a vacuum is attracted.

12133. *Camphine* in the Exp. cell. Object No. 6—distance 0·5 of inch. Five pair of Grove's plates have been used much during the day. The object was repelled with a torsion force of 165°. This would place it between common Alcohol and water (12102).

12134. A torsion of 160° (without weight) had been on the wire all night. Took it off—balanced the beam lightly—allowed it to take its place. It then had lost 2°·5 of torsion. That is, the torsion head had to be advanced 2°·5 to place the beam in its right place. This change has happened since yesterday morning, and I think chiefly during the long interval of the night.

12135. Took a fresh departure by adjusting the Index, Zeros, etc. Used 5 pr. of Grove's plates to the magnet all the day; began to work them about 10 o'clk.

12136. The *beam* alone—balanced by copper wire, etc. is not affected [as to] its place by putting on the magnetic force.

12137. Tried one of the old bulbs of oxygen. No. 1 or oxygen at 1 atmosphere I had given to Magnus. No. 2 or oxygen at half an atmosphere is strongly repelled by the magnet and arrangement; the glass of the bulb being strongly diamagnetic in air.

12138. Employed Object 1 (12035), being a glass cylinder which has such difference in Magnetic capacity, that though it is repelled in air it is attracted in water, Alcohol and Ether (12130); so that I might see how results by *attraction* instead of repulsion came out. The distance was constantly 0·6 of an inch. The rein or bridle was a new card one applied at the object end of the beam. It was attracted to the upper plate of Glass and was moved by moving it, but it was inconvent. to remove it each time the glass was taken away. The experiments were in the following order:

12139. Object No. 1, distance 0·6, 5 pr. plates in

Air	Repelled power		165°
Water	Attracted	„	640°
Com. Alcohol .	Do.	„	490°
Absolute Alcohol	Do.	„	470°
Ether	Do.	„	416°
Ether	Do.	„	410°
Absol. Alcohol .	Do.	„	490°
Com. Alcohol .	Do.	„	505° or 510°
Water	Do.	„	640°
Air	Repelled	„	160°

12140. Here the first and last results coincide very well, except those with Absolute alcohol. So that the mean cannot be very far from truth. It is as in the first column below:

			differences
Air	repelled	162°·5	0
Water	attracted	640°	802·5
Common Alcohol .	Do. . .	498°	660·5
Absolute Alcohol .	Do. . .	480°	642·5
Ether	Do. . .	413°	575·5

12141. Now making air 0 and taking the differences of the others from it, the figures will stand as in the second column.

12142. Reconsider presently these and the former data—now simply enter Saturday's results. Is evident however that whether the Object be attracted or repelled or both, the results are equally correct and valuable.

12143. Took two objects, Nos. 6 and 7, being cylinders of the same glass but one nearly twice the size of the other. Tried them in succession in five different media, *Air*, *Water*, *Absolute alcohol*, *Common alcohol* and *Ether*, in all of which both were repelled, and at two different distances different to the former, namely 0·6 and 0·8 of inch. The battery was 5 pr. of Grove's plates, the same used for the above experiments, so had been used but evidently with little loss as yet, for water at the beginning and the end the same. But as the following experiments took some hours, the battery must have fallen much before the end. The order of the experiments was as follows, and it was ¼ to 6 o'clk. before I had done.

	distance	object	torsion		
In Air	0·6	No. 6	615°	*470*[1]	
,, ...	,,	7	900	*661*	
,, ...	0·8	7	420	*322*	
,, ...	,,	6	300	*237*	
Water	,,	6	25	*22*	
,, ...	,,	7	39	*30*	
,, ...	0·6	7	78	*62*	
,, ...	,,	6	59	*49*	
Alcohol common . .	,,	6	140	*115*	
,, ,, . .	,,	7	185	*150*	
,, ,, . .	0·8	7	95	*70*	
,, ,, . .	,,	6	66	*49*	
					dined here.
Alcohol Absolute . .	,,	6	63	*57*	
,, ,, . .	,,	7	99	· *83*	
,, ,, . .	0·6	7	194	*164*	
,, ,, . .	,,	6	146	*130*	

[1] The figures in italics are in red ink in the MS.; see par. 12145.

	distance	object	torsion	
Ether	0·6	No. 6	185	*153*
"	"	7	207	*208*
"	0·8	7	87	*77*
"	"	6	71	*71*
"	"	6	70	
"	"	7	67 battery fizzing	
"	0·6	7	210	
			accident: glass plate fell on the beam but broke nothing.	
"	"	6	144	121
Alcohol absolute . .	"	6	137	114
" " . .	"	7	157	134
" " . .	0·8	7	89	66
" " . .	"	6	74	51
Alcohol common . .	"	6	55	32
" " . .	"	7	68	45
" " . .	0·6	7	138	115
" " . .	"	6	113	90
Water	"	6	62	39
"	"	7	69	46
"	0·8	7	45	22
"	"	6	41	19
Air	"	6	197	174
"	"	7	248	225
"	0·6	7	445	422
"	"	6	348	325

12144. Now the torsion was examined with a light balanced beam and found to be 23° out, caused I believe by the accident in the Ether series. The index, instead of being at Zero, is at 23° when the beam is parallel to the magnetic axis. Consequently all the numbers since the accident are 23° too high and must have that quantity subtracted from them. This is done in the second column of results.

12145. Can see now, by the comparison of like results, that the battery and magnet has fallen much, and even in the middle of the ether it is falling and was falling therefore before that time. It is hardly fair to take the mean of the extreme results but they are above in red ink[1].

12146. These mean results may for more ready comparison be arranged thus:

[1] Par. 12143, in italics.

	Object No. 6 or smaller cylinder		No. 7 or larger cylinder.	
	distance of Object		distance of Object	
	0·6	0·8	0·6	0·8
In Air	470°	327°[1]	661°	322°
In Ether	317[2] 153°	1·238 256 71°	453 208°	245 77°
In Alcohol absolute	340 130°	1·26 270 57°	497 164°	239 83°[3]
In Alcohol common	355 115°	1·27 278 49°	511 150°	252 70°
In Water	421 49°	1·38 305 22°	599 62°	292 30°

12147. I now before leaving the instrument adjusted the torsion circle and Zero to their standard position and place.

12148. The beam is a bent beam. Thinking that, when the beam lightly balanced and suspended freely, did not have the plane of the beam exactly vertical (which would give a side tug on the wire where it was made fast to the beam, more or less according to the weight), I adjusted it until that plane was perpendicular, according to the indication of two parallel plumb lines.

12149. Observed towards the end of the day that when the N.A. in the cells was much exhausted, on making contact, the force on the object was so much—by continuing contact the battery began

[1] Should be 237°.

[2] Numbers in small type are in pencil in the MS.; those in the smallest type are pencilled between cols. 1 and 2.

[3] A pencilled note pointing to this number reads, "? must be wrong."

occasionally to fizz from evolution of gas against the platina, and whenever this happened, the force of the magnet rose and the repulsion became greater. The acid in that state therefore is liable to generate irregularity, and it should never be used to that degree. The cause of the increase is probably that the evolution of gas causes currents and brings fresh acid against the platina plates.

12150. I ought to have an Object of the same Magnetic capacity as air, and then the effect of the part of the suspension out of the medium would go for nothing.

12151. Must have a curb in the side of the box with an adjusting thumb screw.

12152. Specific Magnetism—Or specific Magnetic Capacity. The condition is quite analogous to Specific Gravity, except that here we have to do with a dual or antithetical power and therefore have no absolute negation or Nil: as in Specific Gravity. The degrees of torsion correspond to the unit of weight in Exp. results on Specific Gravity. Variations in Magnetic force would correspond to variations in gravitating force—and variation in Magnetic force may be produced either by variation of the magnet power without change of place or variation of place without change of magnetic power.

12153. Use a permanent magnet, some time or other.

12154. Use one pole only, some time or other.

12155. Must learn to observe. Must make every thing complete before observing—and must use the battery as little as may be. For this reason must obtain approximative numbers and then use them as points of departure when obtaining corrections for them.

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12156. Have made new lead ring counterpoises for the end of the beam. Also a glass oil lamp for the scale.

12157. New rein of card board, held by a rod passing through the side of the box at the object end of the beam. Appears to act well.

12158. Have adjusted Zero, etc. etc. to parallelism of the beam and zero of the scale.

12159. Revolving the torsion plug (12124); found the same excentricity effect as before and at the same places of the scale. When returned to 0°—all right.

Principles.

12160. When the Object, the Magnetic force and the distance remains the same, but different media are used, then as the object measures out equal volumes of the media, the effects observed express the differences between the object and the media respectively; and though because the object force is not known, these differences are not known, yet because it is constant, the differences between the media are known when two or more are compared with the object at the same time. And whatever the size or the quality of the object may be, the differences between the media will always have the same relation or ratio. These differences are of course obtained by subtracting the less from the larger if the effect is either attraction or repulsion for both; but is given by the sum of the numbers if in one case it is attraction and in the other repulsion.

12161. For if two objects be formed of the same substance but different in bulk or shape, then the series of *differences* will be two sets of number[s], but the same ratio or relative variation, as $\frac{10}{5}$ $\frac{20}{10}$ $\frac{30}{15}$, when of course the one divided by the other will give the same quotient: or as $\frac{6}{9}$ $\frac{8}{12}$ $\frac{16}{24}$ $\frac{20}{30}$, when by division the same constant quotient will be obtained for the particular pair of series.

12162. If the two objects employed have a difference of substance, then though different number[s] will be obtained for the same set of media as before, yet their differences will not vary, but be the same, provided the volume of the two objects be alike; for though the one substance may give numbers as 10, 12, 20, 37, and the other as 15, 17, 25, 42, for four different media, still the differences of these are alike, namely 2, 8, 17. This is because the media, though farther removed from the object or standard in the one case than in the other, are not changed in their relation to each other and therefore their mutual differences come out the same.

12163. So the accuracy or accordance of numbers or differences obtained for the same bodies as media by means of different objects will be ascertained by dividing one set by the other, when a common quotient will result whatever the size or nature of the objects.

12164. Difference of Size is the only point which affects the value of the numbers in the series of differences.

12165. Therefore with the same torsion wire, the same place or distance from Magnetic axis in a given magnet, and the same magnetic force, the difference numbers vary only with the size of the object, and then do not vary in their proportion to each other. Hence they supply an expression of the places of the media in a series and their relative forces, but not of the absolute power or force.

12166. There is not such a standard of absolute force in a dual or antithetical force as in a simple one like Gravity. In magnetism we may take space as the zero and depart in both directions from it, or we may take water or any other bodies; the relative differences between the bodies on both sides of the Zero taken will not be altered by that.

12167. The place of a solid body and its number is easily obtained. If it comes between two fluids, being attracted in one and repelled in the other, the sum of the two forces gives the difference between the two fluids and therefore their respective places, but either of the forces gives the expression for the solid body, i.e. its distance from the fluid whose force is used. In fact the body is for the time *the standard*, or zero, and its number is 0 in respect of the other numbers. Hence the place and expression for a solid is easily obtained by reference to known fluids (12185).

12168. Now for some illustrations. As respects the like results obtained by either attractions or repulsions, i.e. by objects of different *magnetic capacity*. Making air as a zero point for the differences to commence from: the repulsion results at the distance of 0·5 (12102) and the mixed attraction and repulsion results at the distance of 0·6 (12139), for the same media but with different objects, may be taken thus:

	Object No. 6 at 0·5 distance		Differences	Object No. 1 at 0·6 distance		Differences
Air	repulsion	1080°	0	repelled	162°·5	0
Ether	,,	382	698	attracted	413°	575·5
Alcohol Absolute	,,	321	759	,,	480°	642·5
Alcohol common	,,	263·5	816·5	,,	498°	660·5
Water	,,	103·5	976·5	,,	640°	802·5

12169. Now dividing the one set of differences by the other, we have:

Air	
Ether	698 ÷ 575 = 1·21
Alcohol Absolute . .	759 ÷ 642 = 1·182
Alcohol common . .	816 ÷ 660 = 1·23
Water	976 ÷ 802 = 1·21

Shewing by the near equality of the quotient that the two series of numbers are very closely indeed in the same ratio. Indeed, nearer than could be expected for first results.

12170. The results shew also that difference in the *magnetic capacity* of the object or measurer makes no difference in the principle or its results (12160).

12171. Also that difference in *size* of the object or measurer makes no difference in the results (12160).

12172. Also that difference in *distance*, i.e. from 0·5 to 0·6, makes no difference in the results.

12173. And in respect of distance, the two sets of results with the same object No. 6 at 0·5 and 0·7 distances shew the same thing (12102), thus:

	at 0·5	at 0·7		at 0·5[1]	at 0·7[1]
Air	1080°	432°	differences	0	0
Ether . . .	382°	159°		698 ÷ 273 = 2·556	
Alcohol Absolute	321°	112°·5		759 ÷ 319·5 = 2·375	
Alcohol common	263·5	104°		816·5 ÷ 328 = 2·49	
Water . . .	103·5	50°		976·5 – 382 = 2·55	

The last numbers are so nearly alike as to confirm the principle.

12174. The difference in distance is equivalent to working with *different degrees of magnetic force*, and therefore the results above [shew] that such difference of magnetic force at different times, provided it is constant for the same series of observations, introduces *no* alteration or variation of the principle, or interferes in any way with the production of accurate and accordant results.

12175. Again. These differences at 0·5 and 0·7 distance with Object No. 6 may be compared with those at 0·6 distance with Object No. 1 (12168).

Air	0[1] ÷ 0[1]		
Ether	698 ÷ 575 = 1·21	and again	575·5[1] – 273[1] = 2·10
Alcohol Absolute	759 ÷ 642 = 1·182		642·5 – 319·5 = 2·11
Alcohol common	816 ÷ 660·5 = 1·23		660·5 ÷ 328 = 2·10
Water	976 ÷ 802 = 1·21		802·5 ÷ 382 = 2·10

[1] These columns are in red in the MS.

The results are very encouraging as to the practice of the instrument.

12176. With regard to *difference of magnetic power* in the magnet with the same object, distance and media, the results at pp. 2477, 2478 (12102, 12108) will supply a first comparison. The object or measurer was No. 6 ( ) glass cylinder—the distance 0·7 of inch—and the variation of magnetic power that between 5 and 10 pr. of Grove's plates. The results were as follows:

			differences	
	10 pr.	5 pr.	10 pr.	5 pr.
Air	762°·5	432°	0[1]	
Ether	225°	159°	537·5 ÷ 273[1] = 1·968	
Alcohol Absolute	196°	112°·5	566·5 ÷ 319·5 = 1·773	
Alcohol common	173°	104°	589 ÷ 328 = 1·79	
Water	78°	50°	684 ÷ 382 = 1·79	

These again, considering how the battery had been worked, confirm the principle and give assurance of good results hereafter when all is perfect.

12177. Supposing the *torsion wire* changed; then that would cause a difference in the figures or number of degrees of torsion but not in the proportion.

12178. As the force of magnetism varies both with the power of the battery and with the distance from the magnet, it will be easy to correct variation in one of these by variation of the other, and so to select a place always of the same force or to recur to effects at a place of the same force when it is desired to continue a series. Thus with a given object, No. 6 or any other, which may in size or material be made a standard object, if for 0·5 distance it one day give for air 1080° and another day with a stronger battery 1400°, or with a weaker one 790°, the distance only has to be increased or diminished until 1080° results, and then experiments with other media be continued on in that place.

12179. Or if results have been obtained one day with an object and the number in air or any other of the media be taken; another object may be taken another day, and if its place be adjusted until it has the same magnetic expression in the selected media as the former object or measurer, then it may be used to continue the series as if it were the first object.

[1] These columns are in red in the MS.

12180. Beautiful facilities of adjustment, either with different objects or different distances or different magnetic forces or even different torsion wires, are supplied by the apparatus and its principles.

12181. Probably a sphere or cylinder of known dimensions of rock crystal or some natural non-magnecrystallic body would form a good standard. But water or air or space will serve as the standard—the only point to settle is the distance apart; and can easily make that compliable by the decimal point place in the final series, or by using a multiplier for the whole series.

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12182. To-day endeavoured to make some good experiments, using the battery as little as may be from first to last. It consisted of 5 pr. of Grove's plates. The five media were employed—only one object, No. 6 (12035), and two distances, namely 0·6 and 0·8. The experiments were made in the following order:

	0·6	0·8
No. 6 in Air	556°	
,, ...		265°
Ether		103°
,, ...	181°	
Alcohol Absolute	147°	
,, ,,		70° or 71°
Alcohol Common		58°
,, ,,	132°	
Water . . .	64°	
,, ...		29°
,, ...		30°
,, ...	63°	
Alcohol com. .	139°	
,, ,, .		66°
Alcohol Absolute		74°
,, ,,	153°	
Ether	202°	
,, ...		105°
Air		276°
,, ...	584°	

12183. Here the battery and magnet has evidently increased in power from the beginning to the end—probably by an elevation of temperature. In some of the latter observations, that effect of an

increase a little while after contact is made was apparent (12149), as if the act of completing the circuit produced consequences which exalted the force of the battery a little.

12184. Now the mean exp. numbers of the series for the two distances are:

	0·6	0·8	
Air	570°	270·5	2·14[1]
Ether	191°·5	104	1·84
Alcohol Absolute .	150°	72	2·08
Alcohol common .	135°·5	62	2·18
Water	63°·5	29·5	2·15
Object No. 6 Glass	0	0	

12185. Now these numbers ought to represent the paramagnetic forces of the different substances, the object No. 6 being taken as zero, for it is evident that if in a fluid of its own power, it would not move under any force of the magnet, and therefore shew 0° of torsion. Such a fluid or such an object therefore is the zero of the experimental series and the place of the object therefore is known (12167).

12186. So these numbers are the representative numbers, and if those of the larger series are divided by those of the smaller, a number expressing ratio ought to be obtained the same for all, being a multiplier or a divisor to convert the one series into the other, supposing both were correct. They are given above in the red column.

12187. Supposing the experimental numbers quite correct, their differences (   ) ought also to give a common number, but then it ought to be the same as that obtained directly from the experimental numbers. These differences are in no way useful to establish principle, and rather do harm by making the error (which must be error of experiment) seem less.

12188. The extremes of the red numbers differ by 0·34, and ether or 1·84 seems most out. I will repeat the experiments with the same fluid, object and force in another order (12191 *e*), i.e. making all those at the same distance at once, so as not to move the balance to and fro. Probably a source of error there.

12189. Now took object No. 2 (12035), which is larger than No. 6 and of different glass, but still is repelled in all the media (12130),

[1] This column is in red in the MS. See par. 12186.

and ascertained its force in air. It was different to No. 6. Varied the distance until its force in air was 584°, which was the force of No. 6 at the distance of 0·6 at the close of the former experiments. The distance was now not quite 0·7, about 0·68, and it was left unchanged for the rest of the experiments. The torsion wire was more stretched or straightened by this heavy object and the needful counterpoises, as became evident at the telescope by a change vertically in the apparent place of the object. Then took one series of observations for the following media:

Air	584°	584°[1]
Ether	197°	202°
Alcohol Absolute . .	150°	153°
Alcohol Com. . . .	136°	139°
Water	61°	63°
Object No. 2 . . .	0	0

12190. It is astonishing how near these are to the last five observations at 0·6 (12182), which include air as 584° and gave the starting point for these experiments—they are repeated above in red ink. The glass must have been very nearly the same, if not quite, in quality. Where the object is of the same nature but differs in size, the result shews how the series can be resumed by adjustment of the distance (12178), and that no doubt can be done also for difference of magnet power, the object being the same (12179).

12191. When the object has a different character, as No. 1 for instance, then we could not begin at a given number, as in this case, and expect the same series, because the standard or zero would be different from which the distances on either side are taken in degrees of torsion. But still, the suspending wire being the same, and therefore the degrees the same for a given distance from the magnet, the numbers of the media would then appear at the same distance from *each other*—though they would be at different distances from the Zeros of two objects having different magnetic forces.

12191 *a*. The beam was quite right as to position, torsion, etc. at the end of the experiments.

12191 *b*. The experiment ought to be made in a quiet place, for though on a stone floor now, the apparatus is made to tremble by the passing of carriages, and if the beam is just about to separate

[1] This column is in red in the MS. See par. 12190.

from the curb, it sometimes receives a minute blow which disturbs the observation.

12191 *c.* The curb as fixed to the side of the box and made of card acts well.

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12191 *d.* The beam—its place—the zeros, etc. all right this morning. Also the place of the magnetic axis or double cone. The new curb fixed in the box side looks well.

12191 *e.* Believing that in moving the box to and fro from one distance to another disturbed each series by making the distances for each inaccurate, I now left the distance unchanged for each series of two sets of expts., namely, one with Object No. 6 (12035) at the distance of 0·7, and another with the same object at the distance of 0·6. The battery was 5 pr. of plates carefully charged by myself. The experiments were in order as follows:

Object	Air	402°	410°·5[1]
No. 6	Ether	115°	117°·5 or 126°·5
	Alcohol Absolute .	99°[2]	100°
	Alcohol common .	92°	92°
	Water	41°	42°
	Water	43°	
	Alcohol common .	92°	
	Alcohol Absolute .	101°	
	Ether	120°, 130°, 138°.	See further observations ( )
	Air	419°	

12191 *f.* In the first place, the battery by use has gained some power and the magnetic force is greater at the middle and at the end than at the beginning.

12191 *g.* In the next place, the ether presented peculiar actions, as before, and these I believe are due to the currents which are set up in the fluid as it evaporates and cools at the surface and as the cooled portion falls. These currents will evidently rise up in regularity and intensity gradually from the first, and after attaining a maximum will diminish again, but still remain as the air warms the outside of the experimental cell. Hence the cause of the first observation apparent, the increasing amount due to the maximum current and the [illegible] amount towards the last. In the present

[1] This column is in red in the MS. See par. 12191 *i.*

[2] A comment follows this observation: "broke off hook of Object—repaired it—lost time here."

case, when the ether had been so long there as to have arrived at a stable condition, then the observation was 138°.

12191 *h*. One would be able to adjust the cell so that these currents should be either *with* or *against* the balance force, and I must investigate them. A close flannel jacket outside the experimental cell would help to diminish the effect. An inner cell quite within the former and under the ether would much cut them off; with these aids, and leaving the ether until at its steady temperature, I think one would clear off the interfering circumstances.

12191 *i*. So the mean numbers for the results may be set down as in the second column, red ink (12191 *e*).

Then made the distance 0·6 of an inch.

Object	Air	617°	mean numbers	618°[1]
No. 6	Ether	202°[2]	" "	204°·5
	Alcohol Absolute .	142°	" "	151°
	Alcohol Com. . .	139°	" "	145°
	Water	70°	" "	66°
	Water	62°·5		
	Alcohol C. . . .	151°		
	Alcohol A. . . .	160°		
	Ether	207°		
	Air	619°	so the battery nearly as at the beginning	

12192. So these mean results are as follows and by division give the third column of figures—

	at 0·6	*at* 0·7	*multiplier*
Air	618°	— 410°·5	1·505
Ether	204·5	— 117·5 or 126·5	1·74 or 1·61
Alcohol Absolute . .	151°	÷ 100°	1·51
Alcohol common . .	145°	÷ 92°	1·57
Water	66°	÷ 42°	1·57
Object No. 6 . . .	0	÷ 0	

Now these are very well for all except ether—there is also some mistake about air, evidently in the 0·6 series[3], the two observations there being 402° and 419°. If 402° were right, it would make the multiplier 1·53.

12193. Now took a series of single observations of the following fluids with the Object No. 6, and the distance 0·6, being exactly those of the preceeding observations and therefore comparable

[1] This column is in red in the MS.

[2] A comment follows this observation: "unsteadiness as before."

[3] 0·7 series is meant. See par. 12191 *e et seq.*

with them. As by air at the end of them the magnet power apprd. to be very nearly the same as at first, so they are probably worth observation as given [? giving] approximative places for the substances.

Camphine . .	125°
Olive Oil . .	70°; object moves slowly—requires care in such.
dinner→	
Sperm Oil . .	96°
Linseed Oil .	87°
Poppy Oil . .	0°, or nearly so; just moves but scarcely.
Almond oil .	12°
Oil of Lemons .	139°
Air	618°—nearly as before.
Castor oil . .	0°; then the slowest motion—repulsion—oil very thick.
Brine . . .	11° or 10°

12194. In the oil and thick fluids, it requires much care to experiment; the magnetic power must be held on steadily—and very slow motions looked for even then. Some of them, as Castor oil, had better probably be taken in a known vessel, in other media as air or water.

12195. This whole series of results by Object 6 at 0·6 distance may be put together as a list thus:

Air	618°	552°[1]	100	
Ether	204°·5	138°	25	
Alcohol A. . .	151	85°	15·4	
Alcohol Com. .	145	79°	14·3	
Oil Lemons . .	139	73°	13·2	
Camphine . . .	125	59°	10·7	
Sperm oil . . .	96	30°	5·4	
Linseed oil . .	87	21°	3·8	
Olive oil . . .	70	4°	0·725	
Water	66	0°	0	
Almond oil . .	12	54°	9·8	
Brine	11	55°	10	
Poppy oil . . .	0 or 0·5	65?	11·77	
Castor oil . . .	0 or 0·5	65?	11·77	
No. 5 Object . .		61°·3	11·05	(12199)
No. 7 " . .		62°	11·23	
No. 2 " . .		63°	11·4	(12198)
Object No. 6 . .	0	66	11·96	
Pure Zinc . . .		91·6	16·6	(12216)
Common lead .		181	32·8	(12215)

[1] This column is in red in the MS. See par. 12197.

12196. Tried certain of the objects (12035) in air and water for points of comparison at a distance of 0·6 of inch and with 5 pr. of Grove's plates. The following were the results:

				in air	*in water*
	Glass cylinder . .	66 gr.	No. 1	repelled 151°	attracted strongly
	Do.	78 gr.	„ 2	„ 870	repelled 89°
	Do.	115 gr.	„ 3	„ 873°	attracted weakly
	Do.	161 gr.	„ 4	„ 1050°	attracted weakly
	Do.	48 gr.	„ 5	„ 570°	repelled 57°
same glass	Do.	43 gr.	„ 6	„ 617°	„ 62°
same glass	Do.	86 gr.	„ 7	„ 836°	„ 84°
	Glass tube contains	*Water*	„ 10	„ above 1440°	attracted moderately
	Do.	*Oxygen*	„ 12	„ 361°	attracted strongly
	Do.	*Oxygen*	„ 13	„ 388°	attracted strongly
	Do.	*Oxygen*	„ 14	„ 388°	attracted strongly
	Do.	Air Vacuum	15	„ 690°	attracted strongly

The bismuth No. 9 was repelled so powerfully even in water that I durst not carry up the torsion to counteract. Must try this and No. 10 at greater distances.

	in air	*in water*
A portion of copper wire . .	attracted	attracted
Another piece of copper wire .	Do. not much	attracted
A *sovereign* clean	Do. Do.	Do.
A *shilling* clean	attracted much	Do.

12197. The objects 2, 5, 6 and 7 may now have their places ascertained nearly, but it will be as well to assume first the *magnitude* of the degrees of torsion and the *place of Zero*. I will take that magnitude which is given generally above with the present torsion wire when the object is at 0·6 distance, and the magnet urged by 5 pr. of Grove's plates in their first or good condition. It will correspond with the numbers above (12195), and I will take *water* at present for Zero. As it appears in the table as 66, each of the numbers there has to be diminished by that quantity, and then the second or red ink column will come forth as the present representation of the Specific Magnetism of the bodies named.

12198. Then taking object No. 2, which though at the same distance of 0·6 and with the same magnetic power, is larger than object No. 6 and therefore will shew more difference between air and water than it. In air, this object gives 870° and in water 89°, giving a difference of 781°. But with object No. 6, the

difference is 552°. Making these two numbers a divisor $\frac{781}{552} = 1{\cdot}414$ to reduce the number of degrees of No. 2 to that of No. 6 (the standard), it gives $\frac{870}{1{\cdot}414} = 615{\cdot}2$, and $\frac{89}{1{\cdot}414} = 62{\cdot}94$. Taking these as 615 and 63, their difference is now 552°, as with No. 6, as the interval between air and water. So that the series of three things would be

Air	615	or making water 0°, 552
Water	63	0
No. 2	0	63.

Therefore 63 is the expression for the diamagnetic relation of this object.

12199. Object No. 5. In Air 570°; in water 57° (12196); the difference is 513, being less than that of our standard or 552, so they have to be raised. Using therefore $\frac{552}{513}$ or 1·076 as a multiplier, they became for Air 613·3, and for water 61·3, the object being 0°. Subtracting 61·3 from each to make water 0, the numbers then appear as Air 552°, Water 0°, Object No. 5 61°·3. So that it is very near the standard or No. 6 in its diamagnetic capacity. The difference in the experimental numbers was caused chiefly by this condition: it weighs 48 grains whilst No. 6 weighs 43 grains.

12200. *No. 6 object.* In the single and simultaneous trial of No. 6 made with these other standards for a comparison, the numbers came out, in Air 617°, and in water 62°, giving a difference of 555°, which is so near to 552° that it may at present be taken for the same, and shews that the process is a good one, giving the like results again and again and being right in principle. This object is our present standard amongst objects.

12201. No. 7 Object is the same glass but twice the weight and length (12035). Its results are, in air 836°, in water 84°, the difference 752°, due to size. Dividing this by 552 as before gives 1·362 as a reducing divisor, and this brings down the 836° to 614° for air and the 84° to 62° for water. The object then being 0°. Subtracting 62 to make water 0, the numbers are, Air 552°, Water 0°, Object No. 7 62, which is near enough to 66 or the number for Object 6 to be accepted for the present.

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12202. Worked with the Magnetic balance—5 pr. of plates—distance 0·6 of inch.

12203. *Tin*, No. 32—*common tin* in a small cylinder suspended by fine copper wire. Was attracted in air and of course would be so in water. This piece weighed 183 grains, and I have called it as an object No. 32, and hung it in its place.

12204. *Common lead*, No. 29. A cylinder weighing 225 gr. and No. 29. Repelled in *Air*, force 857°—and attracted in *water*, force 212°.

12205. *Zinc*, No. 31, said to be pure—a long piece weighing 136 grains and made Object No. 31. Here an effect came in due to the induced currents on making the magnet, which sent off the zinc with a force= 1080° or more—but continuing the contact, the zinc returned towards the magnet so long as it is retained excited until the torsion force is not 180°. This renders the making and breaking of contact utterly wrong, for each time new inductive currents are formed in the zinc. So kept the magnet excited and then reduced the torsion till the forces were counterpoised at the curb distance of 0·6, and in water at 150°. By using the long handle and adjusting the torsion whilst observing at the telescope, so that I could follow up with torsion the oscillation of the zinc on both sides of its required place, I obtained 115° and think that a good result. As the zinc moves to and fro, currents are produced in it affecting its motion, so that it requires much care to observe, and this must be needed also with the other metals, being good conductors.

12206. The pure Zinc in *Air*. Watched and adjusted the forces altogether at the telescope and obtained 876° and 880° at two different times. I think them good. Again put the zinc in water, to be sure that its position was like that in Air. Observed and adjusted at the telescope—obtained 132°—think that probably about 125° is good expression. The battery is hissing when connected, and then rises somewhat in power. So

12207. No. 31. Pure zinc in *air repelled*, force 878°—repelled in water, 125°.

12208. No. 30, a piece of *common zinc* amalgamated, weighs 157 grains. Repelled in *Air*, 1565°—repelled in water, 330°.

12209. *Sulphur*, No. 34, a crystal weighing 48 grains. Repulsion in *Air* 1922°. Repulsion in *water*, 380°.

12210. *Phosphorus*, *Allotropic*. No. 35, 38 grains—an irregular piece from Schroetter. It was repelled in air and water strongly, namely, in *Air*, 1905°—in Water, 750°.

12211. *Phosphorus*, common. No. 36, weight 41 grains. Five revolutions of the torsion index did not give power enough to oppose the repulsion in air. So I increased the distance to 1 inch and then obtained numbers; the repulsion in *Air* = 491°, and in Water 256°.

12212. *Antimony*, No. 33, weighs 108 grains—a short square prism. Repelled in *Air* = 1085°—repelled in water, 916°. The distance being 1 inch.

12213. Expected that the suspension wire might have gained a set from the high torsion of 5 and 6 revolutions put now and then upon it and with the heavy objects and counterpoises. So after all the experiments, balanced the beam as lightly as possible, and then found that it was 13° out, i.e. when the beam was parallel to the Magnetic axis, the index was 13° in advance of Zero, and when the index was at Zero, of course the beam would be away from the core so much; therefore the first 13° must count as nothing, and that quantity ought to be abstracted from the results obtained since the phosphorus. However, shall leave them as they are and make other experiments.

12214. Can adjust the torsion force very conveniently by means of the long handle at the telescope.

12215. Now some of the results may be reduced. Thus *common lead* No. 29 (12204) in air 857° and in water 212 gives a difference of 645, which divided by 552° gives the divisor 1·17, and this converts
Air 857° into 773°
Water 212 „ 181, leaving the standard or
lead as 0; then subtracting 181° to make water zero gave

Air	552°
Water	0°
Lead	181°

12216. Take *Zinc* pure, No. 31 (12205); in Air 878°, in water 125°: the difference 753° divided by 552° gives a divisor of 1·364, which then makes the air 878 = 643°·7 and the water 125° = 91°·6. Consequently 91·6 is the diamagnetic force of pure zinc.

12217. On the whole, I think it will be better to make Water 0° and Air 100°; the numbers will be smaller and the process of reduction less labour. Then the table (12195) will be[1]:

[1] The list from par. 12195, with many interpolations referring to later work, follows here. (See next page.) The results are in red and the paragraph references in black ink.

Paramagnetism

No. 42. Platina . . .	9300° (12315)
No. 32. Common Tin .	1088°·4 (12310)–245° (12338)
Tin, pure, No. 43 . .	107°·12 (12250)–119°·7 (12289)–123°·3 (12307)
Air	100°
No. 1. Glass cylinder .	77°·72 (12269)
No. 4. Glass cylinder .	45°·43 (12279)–44°·5 (12299)
No. 29. Lead, common .	37°·34 (12248)–40°·3 (12280)–37°·5 (12300) –20° (12204)
Ether	25°–37°·5 (12254)
No. 3. Glass cylinder .	27°·65 (12277)–28°·6 (12298)
Alcohol, absolute . .	15·4
Alcohol, common . .	14·3
No. 41. Zinc, pure . .	5°·77[1] (12237)–10°·37 (12288)–25°·7 (12306)
Oil lemons	13·2
Camphine	10·7
Sperm Oil	5·4
Linseed Oil	3·8
Olive oil	0·725
---Water	0 ———
Almond oil	9·8
Brine	10
Poppy oil	11·77
Castor oil	11·77
Object No. 5	11·05
” ” 7	11·23–11°·2 (12232)–10°·12 (12271)
” ” 2	11·4–12°·44 (12230)–11°·4 (12276)–12° (12297)
” ” 6	11·96–13° (12231)–10°·87 (12270)–43° (12330)–20° (12392)
No. 31. Pure Zinc . .	16°·6 (12216)–1°·5 (12240)–20°·2 (12272)
No. 34. Sulphur . . .	24°·64–22°·2 (12245)–22·34° (12284)–21°·4 (12302)
No. 30. Common Zinc .	26°·72–16° (12239)–20°·23 (12282)–10°·9 (12301)
No. 38. Rock crystal .	28° (12234)–45°·98 (12286)–44°·8 (12304)
No. 37. Borate lead . .	5°·65 (12233)–48°·5 (12273)–43°·6 (12296)
No. 40. Lead zinc tree .	47°·7 (12236)–70°·4 (12287)–90°·8 (12305)
No. 35. Phosphorus, allotropic	65°–61°·1 (12246)–55°·14 (12285)–64°·6 (12303)
No. 36. Phosphorus, common	109°–98°·4 (12247)–78°·4 (12313)
No. 39. Lead by battery	238·5 (12314)
No. 33. Antimony, common	542°–461° (12253)–130° (12312)
No. 9. Bismuth, common	996° (12252)–820° (12309)–1070° (12335)

Diamagnetism

[1] Par. 12237 gives 6°·42.

12218. *Zinc common*, No. 30. Amalgamated (12208). In Air 1565°—in Water 330°—the difference 1235, which divided by 100, now the interval between water and air, gives 12·35 as the divisor for 1565° and 330, making Air 126·72 and water of course 26·72, and so this latter becomes the diamagnetic number of zinc common when water is made 0°.

12219. *Sulphur*, crystallized, No. 34 (12209). In air 1922—in water 380, difference is 1542; divided by 100 gives 15·42, which goes into 1922—124·64 times and into water or 380, 24·64 times; so 24·64 the number for sulphur.

12220. *Phosphorus, allotropic*, No. 35 (12210) gives 65 as its Diamagnetic number.

12221. *Phosphorus, common*, No. 36 (12211) gives so high a number as 109°. Perhaps the other contained iron. Must burn a little of it.

12222. Antimony, common, No. 33 (12212) gives the high number of 542°.

12223. Ultimately consider the following points carefully in their effects:

Difference of distance.
" " size of the Object.
" " magnetic nature or capacity of the Object.
" " magnetic force.
" " torsion wire.

12224. The temperature at which I have worked lately has been from 63° to 65°, etc. To-day it is 63°.

12225. Examined the torsion wire. It was 10° or 11° out when the beam was lightly balanced and parallel to the magnetic axis, and that whether the beam was over the magnetic axis or an inch on one side—that difference had no effect. It seems to have been the result of the continued torsion in one direction. Corrected it and reduced all to Zero.

12226. Whether the beam was lightly loaded or heavily loaded made no difference in its place: it equally set at Zero. No error arose from difference of weight on the wire.

12227. 5 pr. of Grove's plates were used to-day.

12228. When the distance was 0·7 of inch, Object No. 6 was put on the beam in air and balanced as lightly as possible, and the repulsion force ascertained; it was 436°. Then weights were added on both arms until 5 leaden rings (= 230 grains) were added—then though the vibrations were slower, yet the amount of repulsion force was exactly the same as before, namely 436°; yet I set the torsion on whilst observing at the telescope and did not know before hand what numbers I had obtained. So weight to this extent makes no difference in the value of the torsion degrees.

12229. Proceeded to make observations with old and new objects at a *distance of* 1 *inch*, that the wire might have less torsion put on to it. 5 pr. of plates—temperature 63° F. Adjusted the torsion by the long handle whilst observing at the telescope, so as to have results independant of expectation or prejudice.

12230. No. 2 (12035) Glass cylinder. Repelled in Air, 244°, in water, 27°, which by the process (12217), namely of taking the water number from the Air number, dividing that remainder by 100, and using the result as a divisor of the water number, gives 12°·44 as the magnetic place of No. 2.

12231. No. 6 (12055) Glass cylinder, the one chiefly used. Repelled in Air, 165°—in Water, 19°. These numbers give 13° as its place or expression. These result[s] at an inch distance are very good and do not at all burden or strain the torsion wire.

12232. No. 7 (12035). Glass cylinder, same as No. 6 in quality but twice the size. Repelled in air, 248°—in water, 25°. These numbers give 11°·2 as its expression. This is just what it was before, but differs from 13° by 1°·8, and after all an error of 1°·8 is not much where the difference between water and air is taken as 100°.

12233. *Borate of lead fuzed.* Made No. 37. In air, Repelled, 187°—in water, 10°—giving 5°·65 for its number—or in water 9°·5, which gives 5·35 as its number.

12234. *Rock crystal*—a piece. Made No. 38. In air, repelled, 408°—in water, 89°. The number therefore 28°.

12235. *Battery lead.* Made No. 39. Believed to be pure lead free from iron. Repelled in air, 540°—in water, 217°. Hence its number = 67·1.

12236. *Zinc tree lead*, No. 1. Made No. 40—considered also as free from iron. Repelled in air, 402°—in water, 130°—giving its number as 47·7.

12237. No. 41. *Pure zinc*—a fresh piece made No. 41. Repelled in air, 199°—attracted in water 12°. Its number therefore 6°·42.

12238. No. 29. Common lead, same piece as before (12204). Repelled in air, 151—*attracted in water*, 90°. Consider this presently.

12239. No. 30. *Zinc*, *common*, amalgamated (12035). Repelled in air, 255°—in water, 35°. Its number 16°.

12240. No. 31. *Pure zinc*, the former piece again (12205, 16). Repelled in air, 181°—in water 2° or 3° or 0. Number about 1°·5 or 1·2.

12241. Several of the above Objects were heavy in comparison with the Glass cylinder No. 6, etc., but as has been seen, the weight appears to make no sensible difference in the degrees (12228).

12242. The induction effects on exciting the magnet or those produced as the pieces of metal approach to or receed from the magnet are very important, and must be carefully guarded against by continuing the voltaic current and adjusting the torsion force whilst watching. Making and breaking contact cannot be allowed; probably several of the metal number[s] before given are much wrong on this account.

12243. As the battery had been used now some time and I was about to dine, I dismounted it. Found it warm. In the course of an hour recharged it with fresh acid and now observed the effect on the Object last employed, namely the Zinc No. 31 in water. The number of torsion repulsion was now 23°. Wish I had looked also for the power in air.

12244. Went on with the experiments with fresh battery and the old distance of 1 inch.

12245. No. 34. Sulphur (12209, 19) repelled in Air, 363°—in water, 66°—its number 22°·2.

12246. No. 35. *Phosphorus*, *Allotropic* (12210, 20) repelled in Air, 420°—in water, 159°—its number 61°·1.

12247. No. 36. *Phosphorus*, *common* (12211, 21) repelled in air, 506°—in water, 251°—its number 98·4.

12248. No. 29, *lead*, *common*. As before observed (12204, 38). This object gave in Air, repulsion 151°, and in water, attraction of 90°. On the former occasion this sample gave repulsion both in air and water (12204), but I am in doubt now whether sufficient care of the inductive effects had been taken (12242). In the present case the numbers would put the lead as magnetic between Air and water. Therefore when 241°, the difference between air and water, is divided by 100, and the quotient 2·41 used as a divisor of water or 90°, to see how far the lead is off from it in degrees according to the scale, then the 37°·34 obtained for the lead must be taken above the water, i.e. the lead is 37°·34 more magnetic than water and 62°·66 less magnetic than air.

12249. *Platina*. Made No. 42. Pure (12035). This piece, though small in bulk, was strongly attracted—in fact, with or against a torsion force of 620° or more, even when 1·4 or 1·5 of an inch distance from the Magnetic axis. Weighed only 22 grains.

12250. *Tin*. No. 43 (12035). This piece of tin I believe to be pure, at least as regards Iron. It has been precipitated carefully at the Neg. pole of the Voltaic battery, and then fuzed. It was attracted both in air and water and therefore is magnetic to both. The numbers were, for Air, 20°, and for water, 301°. The difference, 281, divd. by 100=2·81, and that goes into 301 or water 107°·12. Shewing that the tin is so many degrees above water in our present scale.

12251. Made the distance 1·5 inches for the purpose of experimenting with *bismuth* and *antimony*.

12252. *Bismuth*. No. 9 (12035)—repelled in air, 570°—in water, 518°, hence its number 996°.

12253. *Antimony*. No. 33 (12035, 12212, 22)—repelled in air, 275°—in water, 226°—hence its number 461°.

12254. ETHER. I have had a double flannel jacket made to the experimental cell. I have also placed a thin glass tube within it as an inner cell. Put ether in the cell and left a while under a glass jar to evaporate and cool. Then used an interval of 0·7 and Object No. 6, to compare the ether with air and water.

The object in Air gave 447° of repulsion—then in the Ether at temp. of 58° it gave 194°, and in water at 63°, 42°; so the degrees between water and air are 405, which divided by 100 gives 4·05, as the divisor to reduce by; then 194—42 gives 152° as the exp. degrees between water and ether, and this divided by 4·05 gives the place of ether 37°·5 above water.

12255. This was the end of the experimental observations—therefore balanced the beam lightly and observed its place, to see if the torsion was affected by the day's work: it had given way a little, but not more than 2°.

12256. The possible change of the torsion during a day's work is a serious evil. Thus it was out about 11° this morning at the beginning and therefore as much last night. Now this makes a very serious error in numbers near to zero, because it affects the place of water as zero. It might even make a body, which is on the one side of water, appear to be on the other even to the extent of several degrees. Thus No. 31 or pure zinc (12240) has its numbers, in air, 181°, and in water, 2° or thereabout. If these numbers were in error 11°, they would be corrected to 192° and 13°. The first set would give the place of Zinc as 1°·12 and the second as 7°·26. Hence probably a part of the difference between the numbers to-day and the numbers of former days.

12257. So Great care required as regards the *torsion and Zero*—and also great care as regards *induction action in the Metals*.

12258. A constant magnet would be a very great advantage, but as the comparison may always be reduced to two or at most three consecutive observations, so the effects of the change of force

during a series of experiments is not so important, because the extremes need not be and should not be related to each other.

12259. The attraction expts. above were made thus. The curb or stop prevents the object from approaching nearer than a given distance, as for instance *an inch* as above (12229). When repulsion is the result of the magnetic force, torsion force is put on in opposition to it until it counterbalances the repulsion, and then the mag. force just frees the object from the stop, an effect seen in the telescope. But in the case of attraction (12238), the torsion force put on was in the contrary direction, and was increased until it overcame the attraction and just carried the body off.

12260. So both attractive and repulsive effects can be observed by the present stop for a given distance. The repulsive action gives a place of stable equilibrium between the torsion and magnetic forces, the attractive action gives a place of instable equilibrium, because the magnetic force diminishes rapidly with increase of distance. A stop in the contrary direction might be used, keeping the object at or *within* a given distance. In that case the instable equilibrium would occur also with the attractive force. There may be practical advantages in using the one stop or the other, or in particular cases using both, since they give observations on both sides of the given distance assumed for the time.

12261. If attraction should, because of the instable equilibrium, give the best results, then a little power might be gained by selecting a medium, but not much, for Water is as diamagnetic as any perhaps.

12262. By looking at the table (12217), it appears that the errors are chiefly among the metals, i.e. the conducting bodies. Must look very carefully so as to prevent the induction effects (12242), for I believe they are a great cause of the differences.

12263. Must consider very carefully not merely the weight or the volume of the body experimented with, but the disposition of the matter—so that if it goes in two or more times, it may go exactly into the same place, as to form even, each time. This is always important, because of the rapidly decreasing or increasing force of the field of action. Must be very careful of this with the gases and their vessels. The reason for having the object and the

suspension in one piece as often as may be is to secure this identity of place.

12264. If a fluid medium equal to air be required, it can be easily made—notwithstanding what Plücker says.

12265. If Plücker were right, that difference of power makes a difference in the result as to magnetic and diamagnetic bodies, that might cause a theoretical error. But I have not found it so.

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12266. Began by observing the beam when lightly balanced. The day before yesterday evening, it was found 2° out and corrected (12255). Since then, whilst out of use the torsion has gone back a little, about 3°, i.e. the torsion index has to be 3° on the near side of Zero (or against the watch hand) when the beam is in the magnetic axis. It appears that the torsion taken under former working and which was kept at the time of the adjustment (12225, 12255) has gradually gone off spontaneously to the extent of 3°. Adjusted it and made all right.

12267. The wire takes up and gives up torsion slowly. Must never leave it except at Zero. Also had better, when weight and torsion are both upon it, return the torsion to Zero before receiving or delivering the beam from the supporting arms to the wire of suspension.

12268. The torsion index and place corrected. The battery 5 pr. of plates. The temperature 64°. The distance 0·8 of inch.

12269. No. 1. Glass cylinder (12035). In air, repulsion, 86° —in water, attraction, 300°. So its place 77°·72 above water or 22·3 below air.

12270. No. 6. Glass cylinder (12035), the same glass as No. 7. In air, repulsion, 306°—in water, repulsion, 30°. So its place 10°·87 below water.

12271. No. 7. Glass cylinder (12035), same glass as No. 6. In air, repulsion, 468°—in water, 43°. Its place 10°·12 below water.

12272. No. 31. Pure Zinc (12035). In Air, repelled, 470°—in water, repelled, 79°. Its place 20°·2 below water.

12273. No. 37. Borate of lead (12035). In air, repelled, 545°—in water, 178°. Its place 48°·5 below water.

12274. The Voltaic battery began to hiss here when connected—an indication of a certain amount of exhaustion and also of a state when its power seemed variable during the time of contact (   ).

12275. The distance was now made 1·0 *inch* because the objects coming on would have required too much torsion of the wire at the former distance. The battery used is the same in its partly exhausted state.

12276. No. 2. Glass cylinder (12035). In air, repelled, 332°—in water, 34°. Its place 11°·4 below water.

12277. No. 3. Glass cylinder (12035). In air, repelled, 259°—in water, *attracted*, 99°. So its place 27°·65 above water.

12278. The first observation in water gave 113°—the next, 100°, the next, 95°. Battery is losing its power, so that the observation in water, being always after that in air, is with less power than that in air; an error here therefore, always on one side. The earliest observations with a battery newly charged ought to be the best. I shall go on however and perhaps take up the same series another day with a fresh battery, beginning at (12273) so as to see the difference.

12279. No. 4. Glass cylinder (12035). In air, repelled, 299°. In water, *attracted*, 249°. Its place by these numbers 45°·43 above water.

12280. No. 29. Lead common (12035). In air, repelled, 142°—in water, attracted, 96°. Its place 40°·3 above water.

12281. Examined the beam and corrected it 1° in the same direction as in the morning (12266).

12282. No. 30. *Zinc common*, amalgamated (12035). In air, repelled, 310°. Went and dined, returned and found the Zinc in air repelled still, 308°, which is a very close coincidence or approach. In water, repelled, 52°. So its place 20·23 below water.

12283. No. 32. Tin cylinder (12035). Attracted in Air at 1·0 with power of 490°, so stopped the experiment to work with a larger distance hereafter (   ).

12284. No. 34. *Sulphur* (12035). Repelled in air, 334°—in water, 61°. Its place 22°·34 below water.

12285. No. 35. *Phosphorus*, *allotropic* (12035)—repelled in air, 342°—in water, 128°. So its place 55°·14 below water.

12286. No. 38. Rock crystal (12035)—repelled in air, 327°—in water, 103°. So its place below water 45°·98.
12287. No. 40. *Lead.* Zinc tree lead, probably pure (12035). Repelled in air, 334°—repelled in water, 138°. Its place 70°·4 below water.
12288. No. 41. Zinc. Said to be pure, part of a plate (12035). Repelled in Air, 121°. In water, distinct attraction and near 14°. So its place *above* water, 10°·37.
12289. No. 43. *Tin*, obtained by the Voltaic battery and I believe pure (12035). *Attracted* both in Air and water. In air, 25°—in water, 152°; so 119°·7 above water.
12290. *Ended.* The beam examined was right. The battery has been very much used—is hot and has crystals of sulphate of zinc forming on the plates. We shall see what amount of uncertainty this introduces.
12291. The receiving fork for the beam vibrates or trembles often as it moves. This gives a tremor to the beam as it is leaving or arriving at it, and this tremor is not good when torsion is on the wire. Would it not be well to cover the fork with something soft, as cloth or flannel?
12292. I think also it is a good rule to reduce the torsion at the index to 0° before taking up or giving up the beam by the fork—as then the jars will not be in the wire when torsion is on it.
12293. A constant magnet is very desirable. Must try ours in its power on water or Glass No. 6. Time is wanting to allow the vibrations to cease, and this time exhausts and varies the battery.
12294. Observations of attraction will be very good with a steady apparatus in a still place, and a constant magnet. With me it is difficult to catch the beam leaving the stop without a jerk more or less, and once having left it, it goes off still further, because of the place being one of instable equilibrium.

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12295. The beam examined; it and the torsion wire all right. Used 5 pr. of plates (much worn) to-day—the distance at first 1·0 inch. The order of the experiments was that in which they are entered.

12296. No. 37. *Borate of lead.* Repelled in air, 214°—in water, 65°. Its number 43°·6 below water.
12297. No. 2. *Glass cylinder.* Repelled in air, 232°—in water, 25°. Its number 12° below water.
12298. No. 3. *Glass cylinder.* Repelled in air, 217°—attracted in water, 87°. Its number 28°·6 above water.
12299. No. 4. *Glass cylinder.* Repelled in air, 274°—attracted in water, 220°. Its number therefore 44°·5 above water.
12300. No. 29. *Common lead.* Repelled in air, 158°—attracted in water, 95°. Its number 37°·5 above water.
12301. No. 30. *Common Zinc, amalgamated.* Repelled in air, 325°—in water 32°; its number 10°·9 below water.
12302. No. 34. *Sulphur.* Repelled in air, 390°—in water, 69°; its number 21°·4 below water.
12303. No. 35. *Allotropic phosphorus.* Repelled in air, 410°—in water, 161°. Its number 64·6 below water.
12304. No. 38. *Rock crystal.* Repelled in air, 433°—in water, 134°; its number 44°·8 below water.
12305. No. 40. *Lead of Zinc tree.* Repelled in air, 458°—in water 218°; its number 90·8 below water.
12306. No. 41. *Zinc, called pure.* Repelled in air, 104°—attracted in water, 36°. Its number 25·7 above water.
12307. No. 43. *Tin, pure, from battery.* Attracted in air, 51°—attracted in water 279°. Its number 123°·3 above water.

12308. Now made the distance 1·5 inches for some stronger cases, and found that hardly enough—would have been better if more for some of the cases.
12309. No. 9. *Bismuth, common.* Repelled in air, 469—in water, 418°. In this case its number came out as 820° below water.
12310. No. 32. *Common tin.* Attracted in air, 553°—attracted in water, 596°. Again in water: only 530°. Again in water, 527°. So the battery appears to be falling; so took it again in air—was 476°. The mean of those in air is 514; and the mean of two in water is 566. This would give the number as 1088°·4 above water.
12311. Much doubt attaches to this result, including (12309) and those on to the end—for the battery is varying and at the end was found falling to pieces, and also the torsion at the end was found

to have lost 7°. Thinking this might have happened with the late heavy loads (12306, etc.) the numbers from (12309), except (12310), are set down 7° less than they read off in the experiments.

12312. No. 33. *Antimony, common.* Repelled in air, 155°—in water, 130°—in air again, 153°. The mean of the air results 154° and that with water give the place of antimony 130° below water.

12313. No. 36. *Common Phosphorus.* Repelled in air, 66—in water 29°; its number 78·4 below water.

12314. No. 39. *Lead, by battery pure.* Repelled in air, 44°—in water 31°. Its place 238°·5 below water.

At 1·75 inches distance.

12315. No. 42. *Platina, pure*—attracted—not much—made the distance only 1·5, then attracted in air, only 51°. Made the distance 1 inch. Then attracted in air 206°, and in water 186°—which gives its number 9300 above Water.

12316. As results of attraction are shewn by a torsion in the contrary direction to those of repulsion, so in this case, and in the other cases of attraction (12310), the experimental numbers were *increased* 7°—but all these latter results are bad.

12317. The beam examined. Was found, as already said, to have lost 7° of torsion—left it uncorrected—but its weight taken off the wire to see the effect of 60 hours rest upon it.

12318. The battery was found falling to pieces by solution of the Zinc. Want a permanent magnet sadly, or a more constant battery.

12319. The induction currents with the metals want much care and attention.

12320. Film on the water. When there was any reason to suppose an effect of this kind likely, the surface of the water was removed, and successfully, by laying a piece of clean bibulous paper on it and thus carrying off the upper layer of water.

12321. As the use of the long torsion handle slightly agitates the balance, I have had a notch cut round it at the end to receive the edge of a plate weighted and having the action of a catch to steady it.

12322. This morning (Tuesday) set the beam free, lightly balanced, and found it 7° out as I left it on Saturday evening (12317); corrected it and set all right.

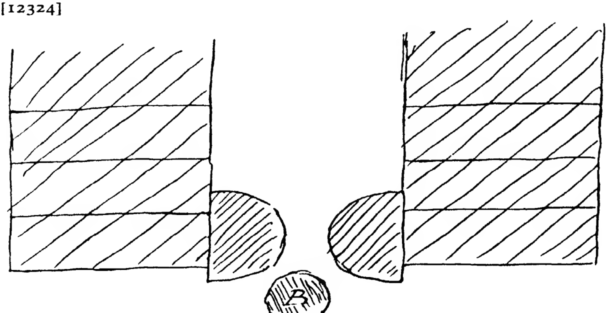
12323. Have made a stop for the long handle near the telescope—it answers very well and is of great use.

12324. Have had a hole made in the side of the lower half of the magnetic box, so as to be able to introduce a common horseshoe magnet, that it may be used instead of the Electro-magnet as a constant source of power. The magnet was supported on the table independant of the box and was adjustible as to distance. It laid horizontal and so that the magnetic axis at the place of the object was in the same relation to the latter as when the Electro magnet was used. The only difference was that the body of the Electro magnet was vertical and beneath the magnetic axis, and here it was horizontal and on one side, i.e. the side from the observer and from the object. Two small iron hemispheres were placed against the ends of the magnet to concentrate the force at the axis and give a more rapid decrease or increase of power. The arrangement is seen in plan of about the real size in the diagram*, B being Object No. 9, or the *bismuth*.

12325. When the bismuth was thus up, the stop being so adjusted in relation to the magnet as to make the distance place as in the figure, then it took clearly 227° or 230° of torsion to counteract the repulsive force there. So that the magnet tells, but the distance is too close for the experimental cell and the power too small for such objects as No. 6, or ordinary fluids.

12326. Put on Object No. 6, Glass cylinder (12035) with a certain near distance, preserved constant for the time—it was affected—put on torsion gradually—left it with 17° on to settle—after a time found the object just home to the stop and I think free from it—dimind. the force to 13° and now the object swung off and kept away and clear of the stop—with 17° again: that is

* [12324]



too much—with 15° the power was full enough—but with 14° the object does not go quite home or settle at the stop, clearly not. So take 15° as the power in *Air*.

12327. Put the Water cell in place (the copper one) and the same object, No. 6, and now the object was close up and against the stop. Reduced the torsion by the long handle. At last the object separated very slowly, quietly and well from the stop, and then gradually continued to go off with an accellerated motion. Found the torsion force was $3^\circ{\cdot}5$. After this, whilst the object out, made the torsion 4°; the object did not go up again. Made it 5°, and then the object closed up and came in contact with the stop, remaining there. The number in water is $4^\circ{\cdot}5$ about.

12328. This result is very small; if it were 8° or 9° it would be much better. All is very quiet when water is used, for the accidental oscillations are destroyed nearly—the operation is slow and requires time and very cautious and intelligent observation. When at the point of adjustment, it is sometimes difficult to know whether the object is resting against the stop or in that place where the repulsive force and the torsion force are truly balanced just in face of the stop. If the latter is the case, short of the stop, then a *little* more torsion is seen to carry the object nearer the magnet *before* the rebound from the stop comes on: if the former is the case, the additional torsion produces a little rebound at once, but then it must also be ascertd. that when at rest, the taking off of torsion causes the object *at once* to receed from the *stop* and magnet.

12329. In the air, the vibrations of the object from the passing of carriages, and the process of the experiments, are very troublesome. When the torsion force is nearly adjusted, the extent of excursion is considerable, for [illegible] of very small force and time is required to allow the motions to subside. But then one has a constânt magnet and no exhaustion of power going on. Perhaps a fluid equal to air in power might be very useful with these low numbers.

12330. As No. 6 has repulsion in air of 15°, and in water of $4^\circ{\cdot}5$, so its number comes out 43° below water. This is far below water according to the former results (12217)—too low. If it had been $1^\circ{\cdot}5$ instead of $4^\circ{\cdot}5$, it would have been 11°. Or if instead of

being 15° in air it had been 45°·5, the number would have been 11°. A very small error near water makes a great error in respect of the places of the object and water. The determining power of displacement is so feeble when the bodies are near each other, as No. 6 and water, that great care is required—perhaps even film may tell here and not tell elsewhere—and perhaps should not observe near water, i.e. things in water which are near to it.

12331. With the *same distance*, put No. 9, Bismuth, on the balance. Brought up torsion to 223°. The object was nearly loose or free of the stop, but it rebounded much from the weight of it and the counterpoise and being in Air. Made it 220°. Think that is about the number, for the returns on rebounding are far and the strokes against the stop feeble.

12332. Placed the bismuth in *water*—brought up torsion to 230° by degrees—reduced it to 225°, observing at the telescope the while—then to 220°—then to 215°—it was out from the stop at the last number and I think also was out before at 220° and 225°. Still, 225° in water and 223° in air cannot agree. Perhaps difference of temperature may have some effect here—or there may not be a clear action in the water cell, for the space or distance is very small. Left all for half an hour. Then whilst observing, advanced the torsion to 221° without knowing its amount, and it still seems as if the object were not at home or against the stop, for more torsion up to 225° carried it further up. At 230° it certainly was quite up to the stop. On undoing the torsion, it went out from 230° downwds. with a slow and steady motion. Still I think the object was touching below in the water against the copper cell before it touched by the beam and stop above.

12333. In Air again at the same distance—214° too little—218° not far out—220 about right, not too much. Made 223° about right.

12334. Now increased the interval, so as to have clear room for the object in the water vessel. Then in air the power needed much reduced—105° too much, yet distance only small—99°·5 I think about right in air. In water—appears to be enough room - 92° nearly right. At 90°, being steady, added a little torsion and the object swung up nearer to the stop; so 90° not enough. At 92° the same action gave no first motion towards the stop as

before. So 92° appears to be about right, or nearly so. The motions are all slow and apparently good. Again at 90°—the object evidently free and carried up by more torsion as before; 90° or 91° cannot be far away from the truth.

12335. So Bismuth in air, 99°·5—in water, 91°. This gives 1070° below water for bismuth (12217) and this probably not far out.

12336. Now employed No. 32 (12035), *common tin*—at the same distance as the last. It was attracted both in air and in water. In air, at 14°·5 held up to the stop against that amount of torsion. When torsion (adverse) increased, it broke up this attraction effect and the object receeded; then opposing torsion being made 10°—the attraction drew the object up to the stop—at 13° still held well to the stop--at 14°·5 stood at the stop, all being quiet—and at 15° went very slowly off but clearly and well. I think this result must have been a good one.

12337. Placed the *common tin* No. 32 in water. There was attraction at 22°; 30°, the object went off slowly, so the true number is between; being off, I reduced the torsion to 25°, 20°, 15° and even 10°, but the object did not return, the attraction at the *greater* distance not being strong enough to overcome the 10°. When the torsion was 0° then the object returned. When the object quiet at the stop, again put on torsion very slowly and carefully; 22°·5 not too much—24°, object stands against the stop. With 25° it goes very slowly and clearly off. Believe 24°·5 about true.

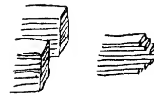
12338. So this tin, No. 32, in air, 14°·5 and in water, 24°·5—being attracted in both cases. Its number therefore is 245° above water, by these data and mode of operating; which is considerably different to the former number of 1088·4 (12310). But I trust this rather than the former.

12339. I then examined the beam balanced *light* and found the torsion of the wire all right.

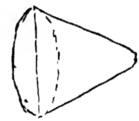
12340. This magnet is too feeble. I must try our magnet of the Saxton's machine.

12341. Beam examined. The torsion was out 9° or 10°, which resulted from its motion on the fork when moving the box so that Newman might work at it. Must have a method of packing the beam more safely, so as to avoid every irregular pull on the torsion wire. Corrected it and made all right.

12342. Our Saxton's magnet appears by the vibrations of a near needle to be two or three times the strength of the Schmidt horseshoe which I have been employing, but it has the disadvantage of flat ends instead of the projecting ends of the former; nevertheless employed it, hoping for more powerful effects, and to test that used only No. 9 or the bismuth cylinder for the day's results.

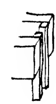


12343. First employed the two soft iron cones—this size—the summits of which were rounded. The magnet ends in plan and their interval are here given* of full size, and the place of the bismuth when at the stop and the force was recorded, is approximately given. Now at this distance the antagonistic torsion was only 30°. The bismuth when approaching the stop did so in a long manner, as if there were only a slowly decreasing or increasing state of power in that part of the magnetic field. What I want is a strong field of power quickly decreasing with the distance from the magnet.



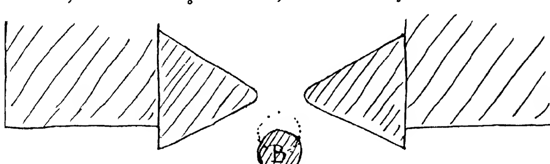
12344. Made the distance less and as shewn by the dotted line, and now the torsion is 100°, which is much better but not equal to the former magnet (12324), and this I think is because of the flatness of the end faces.

12345†. Placed two long square pieces of soft iron 0·5 by 4 inches down the inside faces of the poles, as in the figure. The power was very poor indeed—being only 25°.

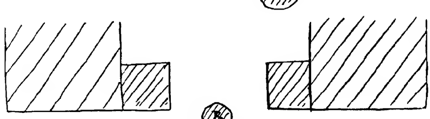


12346‡. Removed these bars and replaced them by other two pieces of soft iron, one a cube of $\frac{6}{8}$ in the side, the other nearly

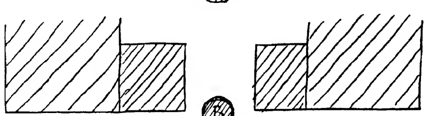
* [12343]

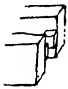


† [12345]



‡ [12346]





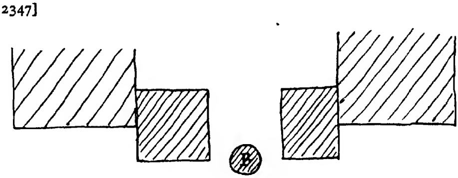
a cube but only $\frac{5}{8}$ in one direction. They were up midway. The power in this case was 218°, but the bismuth was quite up between the cubes or irons. Still, an experimental cell could go in there, and in that respect the form and arrangement is not bad but good.

12347*. I withdrew the magnet $\frac{3}{8}$ of an inch, so as to make the pieces of iron project as it were into the air of the surrounding magnetic field. But the force was less than before, being now not more than 195°. This must be because the bismuth was in some degree withdrawn from the line of greatest force.

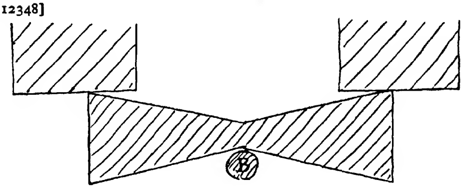
12348†. Put in my long double cone against the faces of the ends. The effect was very poor indeed, only 26°.

12349‡. Replaced this double cone by the thicker one used constantly in connexion with the great Electro magnet, and now the effect was very much better, being 126°. Then added on loose corner blocks of iron at the ends of the double cone, which though fitting very badly, were large, being 1 inch by $1\frac{1}{2}$ by $2\frac{1}{2}$ long; and now the power rose to 275°. It is very important to carry in, by abundance of iron, the forces to the center of the field.

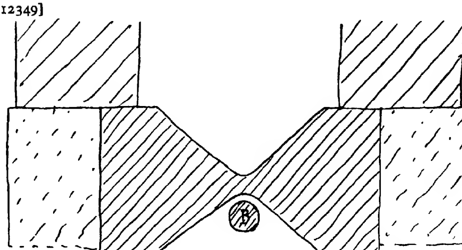
* [12347]

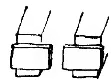


† [12348]



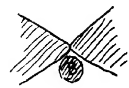
‡ [12349]



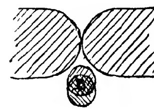


12350*. Now used these blocks as terminations, putting them in front of the end faces thus. The power was only 96°. The depreciation seems due to the large opposed faces at the field of force causing great transmission of the power there and leaving less for the place occupied by the bismuth.

12351. Therefore moved these ends back and placed the cones on thus†; and now the power was 143°. So there was much gain by making   into  .

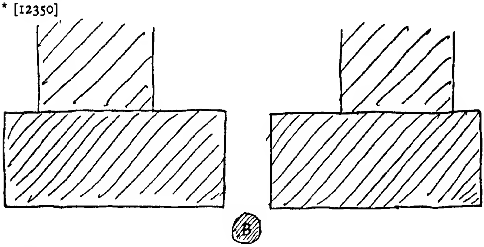


12352. Placed the cones in contact, and then the torsion rose to 486°. But the bismuth was almost in contact with the cones, if not quite. Reduced the torsion to 360°, when its distance away was still very little, but it was quite clear. Even at 180° of torsion, the distance was only small.

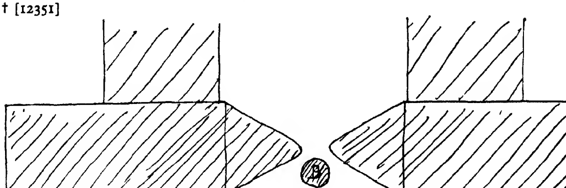


12353‡. Employed rounded ends as in the figure. The effect was good, being 274°. Placed the ends nearer and almost touching—a chip of wood keeping them separate; then the power rose to 462°. Reducing the torsion to 360°, and then the distance out became as represented by the outer form and was enough to allow an experimental cell between.

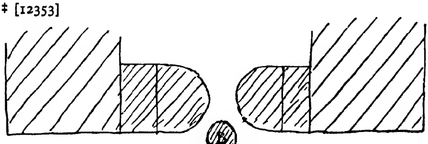
* [12350]



† [12351]



‡ [12353]



12354. The Magnet seems feeble for its size when compared with the former one (12324), so took it to pieces and examined each plate separately. They were all very feeble, and nothing like what they ought to be.

12355. So returned to the Schmidt Magnet (12324), which has also seven plates, the middle plates projecting beyond the external plates, thus*; and the interval between the two limbs or poles being $1\frac{3}{8}$ inches. Put on the hemispherical ends† so as to represent the last case with the former magnet (12353), and with a very good distance had a torsion force of 217°.

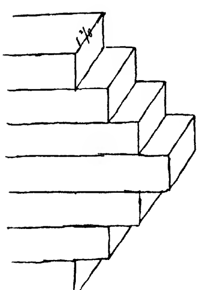
12356‡. Then used terminations, being parallelopipedons, which were $\frac{6}{8}$ deep. At the distance indicated, the torsion resistance was 203°. As the bismuth goes nearer, the power increases greatly above this amount.

12357. So gain nothing by the Saxton Magnet. Must try Logeman's Magnets from Knight's, with good massive pyramidal and short irons.

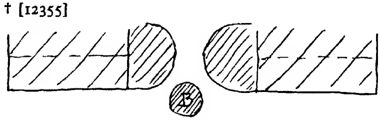
12358. Examined the beam and torsion—all was right, the beam being as lightly counterpoised as possible and having little more than its own weight. Then hung on Object 39, which weighs 267 grains, and counterpoised it by above 200 grains more at the other end, and in this heavy state examd. the place of Zero; it was very nearly the same.

12359. For when torsion $-1°$, so as to leave the beam clear of the curb, when the beam was light it vibrated about or settled

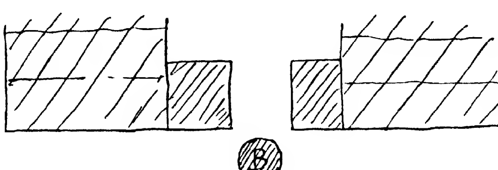
[12355]



† [12355]



‡ [12356]



at 9 in the telescope. When weighted, its place of rest was I think about 11. When the weight was taken off and the light beam examd. again, its place was at $7\frac{1}{2}$ or thereabouts. Now as a degree $= 2\frac{1}{2}$ inches, the distance from the reflector to the telescope scale being 75 inches (12022), so weighting the beam much seems to alter the torsion indication about a degree.

28 AUGUST 1852[1].

12360. Have received the two new stops from Newman and had them put on to the balance box—so as to act on the object end of the beam; they have a screw adjustment and answer well. I have arranged them so as to allow the beam a little motion on each, but one stops by touching on the outside of the beam and the other on the inside or magnet side, i.e. the side away from the observer, and both are adjusted so as to bring the beam up when in the axis of the instrument box, as before (12068). They can be put down out of the way or brought up into action either together or separate, so that the beam can be held stiff in the axis, which is never needed, or observation can be made from a given position in both directions, i.e. by increasing torsion and so bringing the beam up to the inside stop (if the case be repulsion) or by diminishing torsion and so bringing the beam, through the repulsion, up to the outside stop. It will be well to make both observations in every case and take the mean as the true force at the place.

12361. Examined the beam—all was right.

12362. Loading the arms of the support (beam) does not make it vibrate less but indeed more powerfully. Covering them with paper breaks in part the vibration and eases the beam in some degree.

12363*. Have now a Logeman magnet, which can support 150 lbs. It consists of three plates only, the middle one projecting beyond the other two $\frac{1}{3}$ of inch. Its end dimensions are as below[2]. It was introduced through a hole in the side of the lower part of the box, as in the former case (12324).

12364. Either the beam is out a degree, or else being diamagnetic,

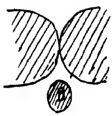
[1] In the MS., 1851 has been written here inadvertently for 1852.

[2] Diagrams in paragraphs 12363–12373 inclusive are reduced to $\frac{3}{5}$ scale.

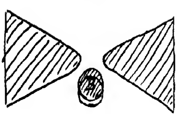
* [12363]

the Logeman affects it a little in its present position—but I left all as it was, the fact not being important to the day's work.

12365. Now arranged terminations by pieces of iron, to see what force was best, building up the terminations sometimes of two or three pieces. Used No. 9, bismuth, as the object, and will try to indicate the distances by diagrams. The first set of ends were rounded and of the size before used (12353), and each end was built up of three pieces of iron. At about this distance*, the torsion force was 668°, which is very good compared with former results with permanent magnets. When the torsion was reduced to 360°, then bismuth stood further out, as figured, but not so much as I expected.

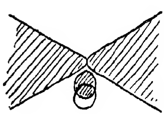


12366. Placed the rounded ends close together and made no other change, and now with 600° of torsion the distance was as figured, and the effect less than before. With 360° the bismuth moved further out, but not better than before.



12367. Employed conical terminations. When thus, the torsion was 720° and very good—for an experimental cell might be introduced here if made of thin copper. With 540° of torsion the bismuth stood out very well. With 360° still better, as shewn by second position.

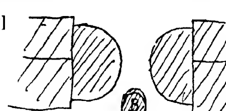
12368. Placed the cones together. The distance appeared to be about the same both for 360° and 720°, and there was fair room for an experimental cell. So as far as bismuth is concerned, the magnet will do.



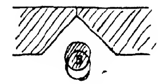
12369. Now employed the Object No. 6, being a glass cylinder, the conical terminations being in contact and just as they were for the last experiment (12368). When only 96° of torsion on, there was not room for an experimental cell. When the torsion was reduced to 40°, then the cylinder stood pretty well out, and there was good room for a cell—but this is in air, and in water the power would have been far less. When the cones were separated by the thickness of a card the cylinder of glass was perhaps a little better out for the 40° torsion.

12370. Returned to Bismuth No. 9 and terminations consisting each of a single bar of soft iron half an inch square and 4 inches long, one end having been filed to a wedge shape thus. Employed these without any accessory pieces as the terminals of the magnet,

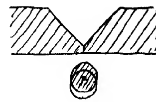
* [12365]



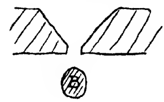
applying them against the outer edge of the middle plate. With 720° of torsion, the bismuth was well separated and would easily have allowed a cell of proper shape. When the torsion was 360°, the bismuth was almost out of the notch. The results were far better than the former ones, and greatly I think because the iron was in single pieces.



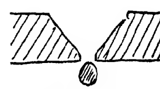
12371. Now turned the terminals round thus: the effect was not so good, for 720° gave as in the figure. When torsion made 360°, the bismuth stood further out and could then easily get round it by a cell.



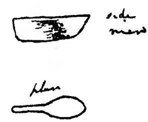
12372. Now opened out the pieces thus, and making the torsion 720°, the bismuth was almost as well out as with the last 360° (12371); and when the torsion was diminished to 360°, it was still farther out, and better than the former 360°. So opening out the ends when of this form and position has very good effect in improving the field of force. Putting the two ends into contact would in fact cause the object to approach.



12373. Employed the same terminations and object No. 6–the ends being open. With 86° torsion, the object did not touch but was about as in the diagram, and far better than before with the former terminations (12365, 7).



12374. I have had a pair of new fish-shaped iron terminations made, more massive than these half inch bars and intended for this Logeman Magnet. When they were put on thus*[1], and the bismuth No. 9 made object, then with 720° torsion the effect was not better than with the bars (12370), or not even so good. With 360° torsion, it was very good. The field does not vary so rapidly in power and this causes the difference.



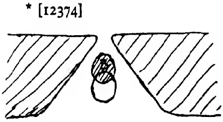
12375†. When the pieces were reversed, 720° of torsion made the bismuth almost touch. When torsion was 360°, then it was better out–but not better than the 720° even, of the former bars open (12372).

12376. When these pieces were opened out, the effect at 360° was very little improved. That at 720° was improved, for object was more clearly away from the terminations. This shape not particularly good except perhaps for position (12374).

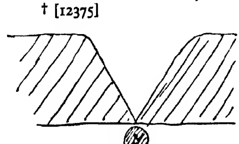
12377. Terminations should be of one piece of iron, and opening

[1] This and diagrams to paragraphs 12375 and 12382 are reduced to $\frac{3}{5}$ scale.

* [12374]



† [12375]



out the ends is good if the ends are square and massive. If the ends were points, then that would imitate the opening out in condition and effect.

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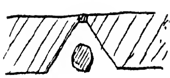
12378. Covered the limbs of the beam holder with many folds of filtering paper, loosely, so as to break vibration, and took off all weight. This improves it as regards the communication of vibration to the beam on raising or lowering it; but touching the outer end during motion is the best damper.

12379. Adjusted two corks so as to hold the beam tight when raised, that it might not slip about on the support, and so the wire become hurt at the lower end.

12380. Applied a lamp and reflector so as to see between the beam and the stops on both side[s], but did not find it very easy to see when the contact was perfect, because of the round and transparent nature of the beam and because of imperfect eyesight.

12381. Examined the beam and made it and the torsion right.



12382. Put the Logeman small magnet (12363) into the box with the bar poles (12370) as represented. Employed No. 6 Glass cylinder, and had the inside stop up, giving a distance about that represented. The needful torsion force directed inwards was now 72°.

12383. Put the inner stop down and the outer stop up. It required much care to ascertain when the torsion force was just able to set the object free from the stop and tend to keep it inwards, because of the short vibrations and the intensity of all parts of the field in which the object was free to move, but I made it again near 72° and think it ought to be a little less, perhaps 70°.

12384. Returned to the use of the inner stop again, always for the same distance, and found the number about 71°.

12385. All this takes time, and the permanent magnet very valuable here. Could not make such prolonged observations nicely or accurately with an Electro magnet.

12386. Employed the water cell for this same object and distance and depressing *both stops*, used the water as a stop only, estimating the distance and the return to the given place by the telescope only. Now avoided all those unpleasant knocks and bangs against

the stop, so that the object took up at once its right position, and yet if in a position, the alteration of the torsion by a single degree was attended by the immediate motion to or from of the object—as seen in the telescope. This process without the stops will be excellent, and if in air the swings be rather free or extensive, it will be easy by a reverse action of the torsion handle to reduce them quickly. All the trouble of the stop—the adhesion—bangs, etc.—the passing of carriages—is avoided. The experiments may be made in Albemarle Street as well as in the country, and if the vibrations in air should be annoying, then it will be easy to make a solution having air Magnetic Capacity and use it for bodies unaffected by it.

12387. In the case of No. 6 in water, the torsion force was 7°·5 or 8°, and the result appeared to be an excellent one.

12388. Returned to air as the medium—using No. 6 and no stops. The object settles very well in its place, and when 6 in the telescope coincided with the telescope wire, the torsion force was only 41°. Now the difference between this and 71° enormous, but then the torsion plug is excentric, and 41° does not coincide with 6 in the telescope, but some other number. When I made the torsion 71°, the telescope shewed 4·85, and when I then put up the inner stop, it knocked at 6·3, as if that were stop distance. Now put down the stop and made the place of rest without it 6·3 by torsion, and the force needed was only 45°. Again made the telescope object 6 and very carefully brought up the torsion and found it to be 50° or 51°·5 or close upon these numbers. So that it is very evident the stop, whether within or without, gives a very uncertain indication for this diamagnetic body in air at about this force and numbers.

12389. The excentricity of the torsion plug is exceedingly annoying, as with the same distance measured out by the stop, it gives a continually varying series of numbers in the telescope, whereas the latter ought to be as constant as the former. To trace this out and remedy it, so that the telescope might register a given distance as surely as the stop, and so do without the latter, the telescope numbers were observed with a light beam and no object but only the stop for every 10° of torsion. The numbers obtained any where in the circle were found to recur in the same

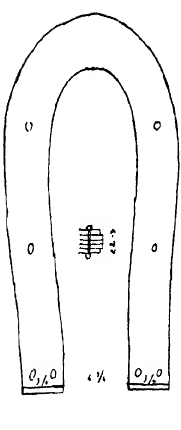
place in the circle; thus 90° and 450° of torsion gave the same number in the telescope. The extreme numbers were found to be at 80° and 260°, or 180° apart, and were for

80°	5·66 in telescope
260	6·5
	0·84, being so much of an

inch or about one third of a degree out (12359). On examining the torsion plug, found the wire hole too large and the wire on that side which would exactly account for this difference, i.e. would exactly coincide with the direction of the difference. So by a piece of card blocked the wire to the other side of the hole—and again went over the observations. Now the extremes appeared to be in the same places, but on the other side to what they were before; thus

70° was greatest	or 6·43
260 was least	or 5·7
	·73

In endeavouring to adjust the card to give a truly central suspension, broke the wire. Have sent the plug to have aperture adjusted.



12390. Examined the torsion wire as to hardness—it will not pull out but breaks; it is as hard by drawing as it ought to be and can be, and is in a very good state (11108).

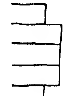
12391. Have sent to Mr Knight for the *Large Logeman.* It weighs lbs. and is said to be able to support lbs. It consists of five plates: the three middle one[s] have an equal flat face but the two outer ones withdraw about 0·4 of an inch, so that the termination has this shape*. The plates are bolted together by 9 bolts in different parts. The magnet has the form represented; is two inches thick or deep and about 23·5 inches long from the outer curve to the edge of the face. The bars have a length at the medium line of $49\frac{6}{8}$ inches.

12392. If we take No. 6 without stops as

giving in Air torsion	45° (12388)
and in Water . .	7·5 (12387)

then it would be at 20° below water.

* [12391]



12393. Placed the large Logeman in the balance box and adjusted it to the expected distance. Looks well and convenient.

12394. Put a new torsion wire on to the beam, the same as the former wire (11108, 12390), having had a very fine hole made in the plug only able to receive it and no larger, for it rubs a little against the sides ( ). The beam hangs well and its bend descends perpendicularly, so as to give no side strain on the suspending wire.

12395. Have removed the glass tube from round the torsion wire and had a copper tube, cut in two halves longitudinally, applied. I can now get at the wire at any time.

12396. When all up, balanced the beam light, brought it by the stop to 0° on the index and to 6 on the telescope scale. Then went round the circle, pressing the beam against the stop by torsion, and observed what the telescope indication was for every 10° or 20°. Found that at 20° it was 6·01, at 200° 5·78, these being the extreme differences, so that 0·23 is now the extent of the excentricity, it having before been 0·84. The present error is because the small hole is not concentric, Newman finding it difficult to make it so. Perhaps the most convenient way to adjust would be by having a hole too large and this partly covered by a sliding adjusting plate on the top.

12397. The following is the present practical result. As the torsion pin and wire are at present, the extremes of difference are at 20° and 200°, and so if 20° be made 6 in the telescope

				in telescope		subtract			
20°	. .	20°	. .	6·00 so subtract.		·00 at	20°[1]	and	20°[1]
0° or 360°	. .	40°	. .	5·98	,,	·02 ,,	0° or 360°	,,	40°
340°	. .	60°	. .	5·96	,,	·04 ,,	340°	,,	60°
320°	. .	80°	. .	5·93	,,	·07 ,,	320°	,,	80°
300°	. .	100°	. .	5·90	,,	·10 ,,	300°	,,	100°
280°	. .	120°	. .	5·86	,,	·14 ,,	280°	,,	120°
260°	. .	140°	. .	5·83	,,	·17 ,,	260°	,,	140°
240°	. .	160°	. .	5·81	,,	·19 ,,	240°	,,	160°
220°	. .	180°	. .	5·79	,,	·21 ,,	220°	,,	180°
200°	. .	200°	. .	5·77	,,	·23 ,,	200°	,,	200°

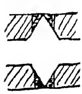
[1] These columns are in red in MS.

Then for further observations at other degrees, the place observed in the telescope is to be that indicated by 6 minus the correction placed opposite the degree: thus at 300° or 100°, 5·90 is to be the place in the telescope, the object being then at the same distance from the magnet as it would be at 20° if 6 were in the telescope.

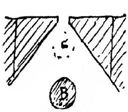
12398. Extreme care must be taken that the telescope is not moved during the observations, as its wire becomes the object in part; and on the other hand the scale at the telescope must not be moved, as it is the other part of the recording object.

12399. If the wire changes in its torsion from set and the torsion scale has to be shifted to preserve the zero, then that will shift the above table of errors also, because they belong to the plug, and belong to it in its present position. Will be well to place pins or permanent marks in the wood at the extremes, i.e. 20° and 200, as now placed, to preserve these places for other states of the ring scale.

12400*. Have had two terminations of soft iron made for the Logeman, of the shape in the diagram, the edge *a* presenting a wedge of an angle of 45° and the point *b* being the summit of a pyramid formed on a similar wedge. The face *c* and the opposite face were finished flat, so as to present fit surfaces to the poles of the magnet, and consequently four different positions could be presented to the object, two with the points and two with the wedge end.

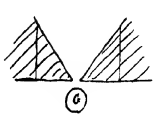


12401. Put up the points inwards or towards the magnet, and the ends $\frac{1}{8}$ of an inch apart, and employed No. 9 *bismuth* object. The effect was good, for with 720° of torsion the object was well out as in the figure.

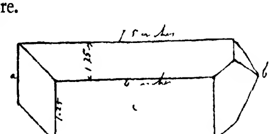


12402. Used No. 6 *Glass cylinder*, with the same poles, open as before. The effect was of course very much less. When the torsion was only 180°, the object was not much away from the poles, about as in the dotted figure. When the irons touched, the effect was better and the cylinder more clear of the poles, so that I could use an experimental cell better.

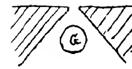
12403. Then turned the poles (pointed) outwards thus; the effect with No. 6 was not better. Closing the poles made a little improvement, as before.



* [12400]



12404. Now used the wedge ends of the poles (   ) and first outwards and open*. With 180° the effect was better than with the points, but not very much; with contact the same nearly. Turned the wedges inwards and when not touching, was better than any of the former position[s]. When touching, better still. Could then have an experimental cell and 360° of torsion with No. 6 in the Air.



12405. This shall be the position for experiment.

12406. Finally arranged the magnet at its proper height, so that the box could freely move so as to adjust the distance. Left the terminal irons in contact in place, but with other bars also of soft iron in contact too, so as to afford an excellent keeper.

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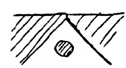
12407†. Have had a new experimental cell (   ) made of thin copper of the size and shape here figured, that it might fit into the angles of the poles. The expansion surface at the top (12070) was made 4 inches by 3 in extent, so as to be quite clear of all capillary action.

12408. Adjusted the plug so that it should be at ·00 of excentricity (12397) and then marked a line across it perpendicular to the position of the beam and made a cross on the end of it towards the handle or observer. This preserves a record of the present excentricity of the plug, whatever place the torsion index may be shifted to.

12409. Copper suspending wire (   ). Made up a bundle of it, and submitting it to the magnet, found it was slightly attracted in air. Must obtain the expression of its true power and place.

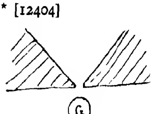
12410. Now worked with this new and beautiful magnet, the poles being as before described (12404). Distce. 0·3.



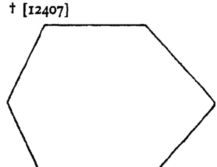
12411. No. 6, Glass cylinder (12035). Distce. 0·3. In air, the required torsion was about 187°, which requd. a correction for excentricity of ·23. This gives the apparent telescope place as 5·77, and at that place the torsion was 204°. This place of stable equilibrium is easily found and kept. The distance at the beginning was made 0·3 of an inch.

12412. The new copper cell had been well washed and soaked in Alcohol and then in water (   ), and being put into it[s]

* [12404]



† [12407]



place filled with water, fitted beautifully. No. 6 in it gave a torsion of 22° at a distance telescopic of 6, which being corrected to 5·98 gave a torsion of 20° for that position. So No. 6 in Air has 204°, in water has 20°, and its place is 10°·8 below water in my centigrade scale.

12413. No. 1 Glass cylinder (12035), distance 0·3; in air (55 about at 6), at 5·96 was 54°. In water it was attracted and so strongly that it was too powerful for the wire. So made the distance 0·5 instead of 0·3, and have kept this greater distance for the other experiments to-day—is better for the experimental cell.

12414. Now No. 1, distance 0·5, in Air was at 6 telescopic place and required 15° of torsion to counteract the repulsion.

12415. As to the observation of the forces when attraction is the effect, no correction of the telescopic place is wanted, for the inner and outer stops being adjusted to keep the beam at 6 when a few degrees of counter torsion are on, more torsion, though it alters the telescopic place, does not alter the object place. First employed the *outer* stop, or that which keeps the object from leaving the magnet, and kept the object there by a torsion greater than the attractive force. Then very carefully diminished the torsion until the object just left the stop, and at first very slowly and then more quickly went up towards the magnet.

12416. The first number thus obtained was 64°. Again, 72°. Again, 64°—the apparent distance being 5·75 in the telescope. For some reason made the distance a little greater by a slight alteration of the stop—it was now 5·9, and the torsion was 57° at this distance when the object left the stop and went inwds.

12417. Now employed the *inner* stop to ascertain this same attraction in water, having about 40° of torsion and the object against the stop at the same 5·9 of the telescope. Then put on torsion against the attraction, and what I may call reverse torsion; and when it rose to 54° the object left the stop and went outwards. Again, then 52°.

12418. After this, to know that the 5·9 distance was right for this inner stop, I took of[f] the object and balanced the beam light, and with 5° of torsion it (the stop) gave the telescope place of 6. So the stop is right both with the heavy and the light beam.

12419. So No. 1, distance 0·5, was
repelled in *air* by torsion of 15°,
attracted in *water* ,, ,, 54°·5,
that being the mean of the results by the inner and outer stops; and this places the object 78°·4 above water.

12420. The difference here between air and water is experimentally only 69°·5. I do not know that it would be desirable to make it greater—if the distance had been 0·4, it might have given a better result. At 0·3 (12413) the force in air was nearly four times greater than at 0·5, shewing the command of range which distance gives.

12421. When *attraction* is the result, then as the position in which the attraction and torsion force is balanced is a place of unstable equilibrium, it is impossible to observe it without stops. The reverse torsion force can never, with the *outer* stop up, except by very careless observation, be given as *too little*, but it may often be given as too much; for beginning with too much, before it is *reduced* to the right point, some tremor may throw the object from the stop a little inwds. to the magnet, and then it will be in a place of more force than its right place, and *may* be carried continuously inwards, though the torsion at the index is a little too great. Again, when the *inner* stop is up, the reverse torsion force can never be given, except carelessly, as *too much*, because it would in every such case, by waiting, go off before it became too much. When the *inner stop* is up, the torsion force begins too small and is *gradually raised.* When the outer stop is up, the torsion force begins too large and is *gradually reduced.* If there were no shocks or vibrations against the stop, both would come to the true force before the object would be balanced:—and therefore *go out* from the inner stop with less force than the true force because the object gets further, or *go in* from the outer stop with *more force* than the true force because the shock send[s] the object nearer than the true distance. With equally careful observations therefore, the mean of the two modes of observation will give the true force for the true place.

12422. *Object No. 2.* Glass cylinder (12035), distance 0·5. Repelled in Air, about 85° at telescope place 6, which corrected gives place 5·88, and for that the torsion force was 83°·5 in air. In water, also repelled, and at 5·98 the force was 12°.

12423. As *film*, if there were any, would act much at these low degrees of torsion, I removed the surface of the water by a piece of clean blotting paper—then restored the object and making the place 5°·98 (for it was not so much after this removal of the film), found the torsion was then only 10°·2. Again took out the object, removed a second film of water, restored the object and now the 10°·2 of torsion raised the place a trace above 6—with 5·98 was at 11°·5, and believe that 11°·5 torsion is a very good expression for No. 2 in water.

No. 2—in Air repelled . . 83°·5
Water repelled . . 11°·5—so its place is 15°·96 below water.

12424. In taking off the surface of the water, one must be careful to remember the agitation produced and wait until the currents have subsided—especially when the immersed object is so near the standard water in its magnetic capacity. The currents are probably as important as the films.

12425. *Object No.* 3, glass cylinder (12035), distance 0·5. In Air repelled, about 78° for place 6. Place should then be 5·9, and for that, torsion was 83° in *Air*. In water, *attracted*. *Inner stop* up—increased the reverse torsion force until at or about 40°, when place being in the telescope 5·85, the object went out; by more careful trials found it went out with 32° torsion—and this not far off. Now the *outer stop* up, and having excess of reverse torsion on, reduced it—at 28° the object went in or up to magnet. Again, 30° is too much and holds it. Again, at 28°·5 very slowly separates and goes in—29° is near—*in Water*—attraction.

So No. 3 in *air* repelled —83°
in *water* attracted—29°—its place therefore is 26° above water.

12426. *Object No.* 4. *Glass cylinder* (12035), distance 0·5. In Air. Repelled, about 100° at place 6, which being corrected to place 5·86—gave then 101° torsion repulsion. In water it was attracted. With the *inner* stop—the reverse torsion was 62°; good, for no shake on. With the outer stop—the torsion force was gradually diminished until 68° was obtained *well*. The mean is 65° for the attraction torsion in water. So in air repelled —101°
in water attracted— 65°—and these numbers give the place of No. 4 as 39°·1 above water.

12427. Object No. 7. Glass cylinder (12035), distance 0·5, same glass as No. 6 but twice the weight. In air repelled, about 72° for place 6, and correcting this to place 5°·93, the force was then 75°. In water also repelled—11°·5 for place 6 or normal place. So the position of this object in the scale appears to be 18°·1 below water. This differs a good deal from Object No. 6 (12411, 2). Must try both again (12429).

12428. *Object No.* 31 (12035), distance 0·5. *Zinc, supposed pure.* In air repelled—true place 5·93 and the torsion required, 68° in air. In water just repelled—its place 5·93 and the torsion 10°·5 in water.

12428½. Was led here to verify the torsion of the wire with a light balanced beam, and found a change of 4°·5—to be *taken off direct torsion* and *added* on to *reverse torsion* for the last two observations, and it may be for others, as those of Nos. 4, 3, 2, etc. Corrected this torsion for the further observations.

12429. Now No. 7 above (12427), distance 0·5, being corrected by subtracting 4°·5 from the results, gives in Air 71°·5
in water 7°·0, and this
gives the place of No. 7 as 10°·85 below water, which is almost identical with that of No. 6 (12412).

12430. Again No. 31 (12428), distance 0·5, appears to be in Air 68° and in water 10°·5, which being diminished by 4°·5 (12428½) gives repulsion in air 63°·5
repulsion in water 6·0, and these numbers
place this Zinc at 10°·4 below water.

12431. The wire is now *correct.*

12432. *No.* 37. *Borate of lead fuzed* (12035), distance 0·5. In Air repelled, and at corrected place of 5·9 with force of 87°. In water, also repelled, with a force of 30°·5. So repelled in *Air* 87°
repelled in Water 30·5, and
therefore its place 53°·1 below water.

12433. *No.* 38. *Rock crystal* (12035), distance 0·5. In air repelled, and at the corrected place of 5·8 with a force of 159°. In water repelled—with a force of 54°. Took off a film of water and replaced the quartz and now the place was 53°. So in Air 159°
water 53°; and the place in the scale 50° below water.

12434. *No.* 29. *Common lead* cylinder (12035), distance 0·5. In air repelled a little, at corrected place of 5·98 was with force of 20°·5 in air.

Attracted in water. Inner stop up gave 54° as final force—the outer stop up gave 65°—and the mean is 59°·5 attraction in water. Examined the torsion and found it a little out, requiring 1° to be added to direct torsion or taken from reverse torsion. So lead was

in Air repelled 21°·5

in water attracted 58°·5; and this gives the place of lead as 73°·12 above water.

12435. Torsion corrected at the index plug.

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12436. Broke the torsion wire first thing this morning. Had no length of new wire sufficient, so soldered two ends on to a piece of copper wire, winding them first round it neatly enough and sound enough to carry any required weight; the place was about this size.

12437. When all was in place, I balanced the beam with a heavy charge and adjusted torsion to 0° or zero. Found that the wire altered as I watched, and by degrees made out that it rotated in one direction, i.e. that the beam continued to go slowly round, so as to alter its zero place, and though the effect was small and slow, it was destructive of any use in the wire. I think it must have been from some slow settling of the wire w[h]ere it, being spirally wound, first entered the solder at the joint. Took it down, and have ordered new wire from Newman.

12438. Cured the vibrations of the fork by passing a string round it at the socket and making the other end fast to the box, so as to give a little horizontal side pull on the fork. Shall now remove the covering paper.

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12439. Put a new torsion wire into the Instrument with great care. It was as the former (11108, 12390). Broke off the hook at the end of the beam. Took the beam out, restored, i.e. remade a hook, and restored the beam to its place; all appeared to be

right. When lightly laden, it appeared to swing properly. Have now unpapered the arms of the receiving fork to observe better.

12440. The excentricity of the wire appears to be now very small, being only 0·08 in the telescope, and this appears due to the fact that there is no sudden flexure or sharp curvature near or close to the torsion plug. The extremes occur now very near or close to 0 and 180, so that I have marked them with ink on the plug. Six intervals will be enough in the correction tables, taking the difference for these intervals as 1, 1, 2, 2, 1, 1.

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12441. When the beam was light this morning and in place, the Zero was a little out. Adjusted the torsion scale first and then adjusted the telescope at 6. Then adjusted the *inner* and *outer* stops to the telescope at 6. When the beam was balanced heavily laden, its place appeared to be the same, but the vibrations were very slow.

12442. With the inner stop up and direct torsion against it, observed the excentricity of the beam: it had its extremes nearly at 0° and 180° of the torsion index, being for 0°–place 6
180°– „ 5·92.

I have thought the correction might be made at every 30°, and of course has to be subtracted when 0° is made 6, thus–for the observed place when

torsion is 0°	or 360°	subtract	·00 from 6
30°	330°	„	·01
60°	300°	„	·02
90°	270°	„	·04
120°	240°	„	·06
150°	210°	„	·07
180°	180°	„	·08

Proceeded to observe.

12443. No. 6, Glass cylinder (12035), distance 0·5. The present distance was too great for this small object, being 0·5 (12413), and the observation becomes very delicate when the body is so near water because of the extreme slowness of the motion and the necessity of avoiding currents in the air or water, or other causes of error. In Air it gave 45°·5, and in water 6° or very nearly so,

being repelled in both cases. This gives its place below water as 15·2—distance 0·5.

12444. No. 4. Glass cylinder (12035), distance 0·5—in air repelled 88°. In water it was attracted. With the *inner stop* up, put on reverse torsion until the object went out—55° too much—50° not enough—finally made the number 52°. *Outer stop* up with too much reverse torsion—then diminished it—65° held it clearly—62°, it goes off—or stands—goes of[f] at 62°. Made the final force 60°. So in Air No. 4 is repelled with force of 88° and in water attracted with force of $56° = \frac{52+60}{2}$. Perhaps these numbers ought to be altered to air 87° and water 57° (12447). The first pair of numbers gives *No.* 4 as 38·88 above water, and the second or corrected numbers as 39·5 above water.

12445. No. 3. Glass cylinder (12035), distance 0·5. In air repelled 73°·5. In water attracted. With the *inner stop* up—judge the best No. to be 26°. With the *outer stop* up obtained 29°. The average is 27°·5. So the numbers are

In air repelled 73°·5 or if corrected (12447) 72·5
water attracted 27°·5 " " " 28·5; and

so the place of No. 3 is either 27°·2 or 28°·2 above water.

12446. No. 2. Glass cylinder (12035), distance 0·5. In air repelled 73°·5. In water repelled 7°·2. So the place is either

Air 73°·5 or else (12447) 72°·5
Water 7°·2 " " 6°·2; and having no

doubt that the latter is right, we have the number or place of No. 2 as 9·35 below water.

12447. For here I verified the torsion index with a light beam and found it out 1°. Zero was advanced to 1° of direct torsion, so that 1° has to be taken of[f] the last and probably the preceeding numbers. Left the zero place correct.

12448. *No.* 30. *Common Zinc, amalgamated* (12035), distance 0·5. In Air repelled with 79° of force. In water attracted. The inner stop gave 9° or 9°·2 as the reverse torsion, and the outer stop gave 12° or 11°·8, the average being 10°·5. These results give the place of No. 30 as 11·7 above water.

12449. *No.* 34. *Sulphur* (12035), distance 0·5. In air repelled 123°. In water repelled 23°, giving place of sulphur below water as 23.

If the correction (12452) applies here, then in air it is 122°·2 and in water 22°·2, and that gives the place of sulphur as 22·2 below water.

12450. No. 35. Allotropic phosphorus (12035), distance 0·5. In air repelled 123°·5—in water repelled 45°·5. Its place below water 58·3. If the correction applies (12452), then it is in air 122°·7—in water 44°·7, and the place 57°·3 below water.

12451. *No.* 36. *Common phosphorus* (12035), distance 0·5. In air repelled 135°·7—in water repelled 69°. Its place 103·4 below water. If the correction (12452) applies, then in air is 134°·9—in water 68°·2 and the place 102·2 below water.

12452. The beam was again lightly equipoised and examined, and the torsion found 0°·8 out, to be taken of[f] direct torsion and added to reverse torsion for the last case and perhaps another or two back (12449, 50, 1). Left corrected.

12453. *No.* 40. *Lead from Zinc tree, pure* (12035), distance 0·5. In air repelled, 162°—in water repelled, 68°. Its place 72·3 below water.

12454. *No.* 41. *Zinc pure* (12035), distance 0·5. In air repelled 48°—in water it was attracted. By the *inner* stop 14°—by the outer stop 13°, the average 13°·5 reverse torsion in water and 48° direct torsion in air. The place therefore 22 above water.

12455. Verified the beam and torsion—all right.

12456. The results with the Electro magnet cannot yet be depended upon, for I have learnt how to observe since its use, but there are some results which appear still to be distinct and important. No. 30 Common Zinc, very impure and heterogeneous, and rendered more so by the mercury in and about it, was repelled in water by the Electro magnet (12239, 82, 301), but attracted in water by the large Logeman Magnet (12448). Is there any mistake in these results, or is it a fact dependant upon the difference of power in the magnets, coming under Plücker's observation (Exp. Res. 2633)? And if the stronger magnet repels what the weaker magnet attracts, does that depend altogether on the heterogeneity of the common zinc, and is it absent with pure Zinc, which is repelled by the Logeman as well as by the Electro magnet?

12457. Common Zinc is below pure zinc by the electro magnet

(12217); now that can hardly be by any affection of the iron in the common zinc.

12458. If there be a difference by different powers, it ought to affect the mutual relation of a body and the medium surrounding it, as well as a body containing impurities within itself; so that air and water may have to be considered in my experiments.

12459. To guard against any such effect, it would be well to make all the experiments for the table with an invariable magnet—and at an invariable distance.

12460. If there be such an effect—the results at different distances from the Logeman ought to shew it.

12461. No. 6 with the great Logeman, at the different distances of 0·3 and 0·5, gives very different numbers, namely 10·8 and 15·2 below water (12412, 43), but the last observation is very delicate. Must repeat this result with No. 6 and No. 7 at other distances—to get the true habits of glass in this respect. For it is very important as regards oxygen.

12462. Pure Zinc, No. 31, appears to be very variable with the Electro-magnet (12217): has that also any thing to do with this variation of force?

12463. No. 2 with the great Logeman, at the same distance of 0·5, is at one time 9·35 below water and at another time nearly 16. What is the cause of this difference, and how far has it, which could depend upon no difference of strength of magnet or distance, affect[ed] the other results just referred to?

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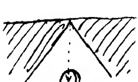
12464. With the beam balanced light and in place, made the telescope standard 6—and placed a bob line at 4 for the purpose of a record for the day—for correction in case of accidental displacement of the scale. The distance is still 0·5. On examining the place of the poles, however, found the distance from the vertex of the angle to the center of the object about a 20th of an inch more than that, for the angle is so far beyond the standard mark on the table, and therefore all the distances have to be increased (if necessary) by this constant quantity.



12465. No. 32. Common Tin (12035), distance 0·5. Attracted in Air. Inner stop up and adjusted to the telescope place 6 at 0°

of torsion. Then reverse torsion on; rose up to 650° at this distance. This was too much for the wire, but as it was on, put up the outer stop, adjusted it to 6 when balanced light and with 5° of reverse torsion, and then put the tin in place and experimented. I now raised the reverse torsion to 2 revolutions and 85°, i.e. 805°, and still could not hold it against the stop—though 650° was enough before.

12466. There appeared also to me a certain unsteadiness—as if the tin took at times a polarity and kept it a little—and this by fits and starts, according as it happened to swing nearer to or further from the magnet. The object wants a special study in this respect and in relation to that *point of action* generally. If there be any such assumption and retention of a feeble magnetism-polarity, it must make the results very uncertain till it is discriminated (12470).

12467. After this, made the beam light and found that the torsion had given way 10°, Zero having gone so much reverse: hence the measurements above would have to be diminished 10°.

12468. This set of the wire is very natural—and is accordant with all the phenomena observed. Much torsion either direct or reverse tend to make the wire give way, and this giving way or set is greater as the weight on the wire is greater and as the tremors from passing carriages or other causes are more. Must make frequent correction. Left torsion corrected and right.

12469. Have applied a thin copper shield to the faces of the poles when in air, to prevent contact of the objects with the iron.

12470. *No.* 43. *Pure Tin* (12035), distance 0·5. In air attracted. Inner stop up and made 6. Tin goes off at 15° reverse torsion; gradually corrected the number to 12°. With the outer stop up, gradually diminished the reverse torsion to 17°—16°. With the inner stop up again, and great care, rose up to 14° and I think nearly 15°. So there are no signs of a polarity assumed here (12466). I shall take the mean number as 13°·4 reverse torsion in air.

12471. In water, No. 43, distance 0·5, attracted. Inner stop up—reverse torsion was successively increased to 88°—then 97°, too much, and gradually reduced to 92°. When the outer stop up, excess of reverse torsion was gradually reduced to 113°—111°—103°—mean 97°·5. The torsion was now examined and found 1°

out, to be taken from direct torsion and added on to reverse torsion. This could not have been given by any amount of reverse torsion, nor by the small direct torsion, except that if a transient reverse torsion set had remained from the former experiment (12467), a little direct torsion with a heavy weight and tremors might take it back the degree. So shall make the correction for both; then No. 43 is in Air, attracted, torsion force 12·4
Water, Do. 98·5; its
number therefore is 114·4 *above water*. Torsion left right.

12472. *No.* 39, *pure lead* (12035), distance 0·5, heavy, weighing 267 grains. In air repelled 203°. In water repelled 83°·5. Torsion examined, correction 2° to be taken from direct torsion. As this must have been given during the large amount of torsion whilst the lead in air, I shall take it off both. So in Air, 201
in Water, 81·5;
and the place of lead 68·2 below water.

12473. Now increased the distance to 0·7 by the table lines, for *Bismuth* and *Antimony*. No. 9, Bismuth (12035), distance 0·7. Repelled in air, 309°. In water repelled, 290°. Weight of the object, 104 grains. The *Antimony No.* 33 (12035), weight 108 grains, distance 0·7, was repelled in air, 156°·5—and also repelled in water, 127°·4. Now examined the torsion and found it 1°·8 out, to be taken from direct torsion. Now as the weights are nearly equal, I think I should assume this change as gradually coming on in proportion to the torsion on the wire, and as that of bismuth was twice that of antimony, so I shall assume 1°·2 and 0°·6, or rather 0·6—0·6—0·3—0·3, and therefore take 0·6—1·2—1·5—1·8 from the successive observations. This gives the following results:

No. 9 Bismuth $\left\{\begin{array}{ll}\text{in Air} & 308^\circ{\cdot}4 \\ \text{in Water} & 288^\circ{\cdot}8\end{array}\right\}$ its place below water 1473·4.

No. 33 Antimony $\left\{\begin{array}{ll}\text{in Air} & 155^\circ \\ \text{in Water} & 125^\circ{\cdot}6\end{array}\right\}$ its place below water 427·2.

12474. Now took the *common zinc*, *Amalgamated*, *No.* 30 (12035), distance 0·7. It was at this distance 0·7 repelled in air, torsion 30°·5. In water, attracted feebly. Inner stop. Reverse torsion, 7° not enough—9° too much—8° settled at. Outer stop up—first reverse torsion 9°—then reduced to 8° and I take 8° to be the true

number as the mean. One sees here how nearly the observations by the two stops approach when the body is nearly like water, and that naturally, for then vibrations do not tend to throw the body into places of much different force; but one has to take great care of currents in the fluid.

This object in air repelled $30^\circ{\cdot}5$, water attracted 8° } its place $20^\circ{\cdot}78$ above water.

12475. *No.* 31. *Pure Zinc, long piece* (12035), weight 136 gr. distance 0·7. In air slightly repelled—torsion 26°. In water attracted. Inner stop up—finally made the reverse torsion 2°. The outer stop up again, made the reverse torsion 2°. Verified the torsion and found it right—for there has [been] no great deflection either with or without weights. Hence

No. 31 { in air, repulsion 26°, in water, attraction 2° } and its place $7^\circ{\cdot}14$ above water.

12476. No. 2, Glass cylinder, at this distance 0·7. In air repelled 36°. In water repelled very little—almost as water. Direct torsion 2°—it goes off—with 3° it crept slowly up to place 6·4 and then retreated a little with $3^\circ{\cdot}5$—left it and went to tea and afterwards found it had gone back to place 8. Increased the torsion to 5°—it went to place 7—made torsion 6° and then it went to place 5·9, and being left a while slowly settled at place 6. Now all this I suspect was due to feeble currents in the water, which could influence a body so near to water and at the same time so far off the magnet as to be in a part of the field of comparitively slow decrease in power. Taking the data as:

No. 2 { in air, repelled 36°, in water, repelled 6° } its place was 20° below water.

12477. The torsion wire was all right.

12478. There were no signs of film in these experiments.

12479. The temperature to-day about 69° F.

12480. *The Stops.* Direct motion of the adjusting thumb screws makes the stops arrest the beam at *lower* numbers in the telescope.

12481. *Glass Experimental cells*: one being put in place at this distance of 0·7, gave plenty of room for No. 6 cylinder to hang freely in it—and it would do so with a distance of 0·6 and I think also of 0·5.

12482. Torsion all right. Place made 6 in telescope and by stops. The temperature 65°–64°, etc. Oiled the large Logeman magnet. Arranged for a close comparison of Nos. 2, 6 and 7, being Glass cylinders at different distances from the magnet–to determine the mere effect of distance–and began with the great distance of 0·7.

12483. No. 2 (12035) at 0·7 distance. Great Logeman Magnet for the whole day. In Air repelled 30°·6. In water, repelled but very little, and now the effect of slow movements in the water and the need of time and careful observation came in. With torsion 3°, its place was 6–in three minutes after it was 6·3–in 10 minutes after still 6·3. Examd. the index of torsion carefully and set down the force as 3° or 3°·1. So its place by this expt. is 11·3 below water.

12484. No. 7 (12035) at 0·7 distance. In air repelled 28°·8. In water repelled, was 2°·8 or 2°·9 at place 5·8–in 8 minutes was at place 5·92–in 5 minutes more at place 6·01–after a while came to 6·05. So take the torsion force as 2°·9. So the place by this experiment is for No. 7 11·2 below water.

12485. Made the distance 0·5, leaving No. 7 in the Exp. cell and all things else as they were. So No. 7 in water at 0·5 distance. The torsion force had to be increased–when 6° the place was 5·8–in 5 minutes it became 5·85. May call the torsion force 6·1 upon close examination of the Index. Now in air. The torsion force 61°. Hence the place of No. 7 appears here to be 11·1. This very close to the result at 0·7 distance.

12486. No. 2 (12035) at distance of 0·5. In air, 69°. In water, 7°·2 at place 6·05–which gradually became place 6. So take 7°·2 as the force–here therefore No. 2 appears as 11·6 below water–before it was 11·3, which is very close (12483).

12487. Diminished the distance to 0·4, leaving No. 2 in the water as before (12485). The torsion force was increased to about 13° at place 6–which in 5 minutes became place 5·83–in 4 minutes more, 5·73–and so went slowly to place 5·62–the torsion carefully examd., being 12°·8. As the place was steady and too near (5·62), diminished the torsion to 12°·6, which (except for the momentary shake) caused scarcely any change. Made the torsion 12°·4, and now the place changed and became 5·86. Left it for

half an hour—but by an unlucky touch of the nose on the telescope, disturbed its place and could not go on further. Shall assume 12°·4 as the right torsion force in water. Now No. 2 at 0·4 distance in *Air*—finally 82°. This gives the place as 17·8 below water. This is a serious difference from the two former results, and either shews some effect of the derangement at the telescope or some difference at different distances.

Examd. torsion, zeros, etc. and left all right.

12488. Diminished the distance to 0·3. No. 2, in air—torsion 150° with place 5·93. Final torsion, 153°·4. In *water*, torsion 16°·5 at place 6·08, which 5 minutes after was still 6·08. So that torsion force was 16°·5 or 16°·6. This makes place of No. 2 12·1 below water—which is not far from the first and second results at 0·7 and 0·5 (12483, 6), but much away from the third result at 0·4 (12487), so that there must have been some derangement of the telescope there for the second operation, or some other disturbing effect (12494).

12489. No. 7 at distance of 0·3. In air, 137°·8 of torsion. In water, 14°·3 at place 5·9. Finally estimated the right force as 14°·1. This gives place of No. 7 as 11·4 below water, which is close to the former results (12484, 5).

12490. Verified the beam.

12491. Began a series of observations with No. 6 (12035) at different distances. First this distance of 0·3. No. 6 in water—torsion 11° at place 6·06—in 5 minutes place was 5·91. Made the torsion less, or 10°·8—place 6·05—in six minutes place was 5·95. Assumed 10°·8 as the correct number of torsion force. In *Air*, the torsion force was 94°. So the place of No. 6 was 12·98 below water.

12492. Distance made 0·4. No. 6 in air, torsion 55°. In water, finally 7°·3. So by these numbers the place is 15·3 below water. The numbers in water here had been as high as 7°·8, 8°·5 and 8°·3 and were then gradually lowered in long time to 7·3. Any of these numbers would have given a place still more beneath water than 7°·3.

12493. Distance made 0·5. No. 6 in water—torsion 4°·8 at last. In air. *Of a sudden all wrong*, and I see not why, but even with reverse torsion of 20°, the object stood at 6° as if attracted and

yet not as if in a place of unstable equilibrium. It seems as if something were pulling both against the reverse torsion and the diamagnetism, and also as if the beam moved irregularly and slowly about place 6—as if indifferent to air—or had assumed a state—or was embarrassed.

12494. Suspected a cobweb—so carried a quill feather round the beam as well as I could—and now it seemed freed in some degree. Made the beam light and equipoised. The torsion was right but the beam moved more slowly than it ought. I believe a minute spider has got on to it, and has affected the results from (12488) onwds. I must go well over the wire, beam and inside of the balance box. Such a thing requires great care.

12495. Half an hour after—finding the light beam at Zero—put on direct torsion 10°—it moved slowly but correctly in direction. Hung on No. 6 for a general observation in air—it was repelled as it ought to be and gave 35° torsion for the place 6. These numbers of air 35° and water 4·8 give the place 15·8 below water for No. 6—but neither of the numbers can be trusted.

12496. With regard to the currents in the liquid. I have put a double paper jacket round the water cell to keep its temperature constant. I think it good always to move the cell to and fro[m] the poles as to keep it parallel to itself. Evidently much care is required at *low tensions* in respect of the habits of the liquid.

12497. Very much care is required as regards spider's webs. Must test the condition by observing first thing evy. day of experiment what is the rate of vibration when at 0° and light—and whether 5° torsion direct or reverse carries the beam 5° to the right or the left.

12498. So the places for the objects Nos. 2, 7, 6, appear by these experiments to be as follows, being obtained at different distances:

Distances	0·7	0·5	0·4	0·3
No. 2	11·3	11·6	17·8	12·1
No. 7	11·2	11·1		11·4
No. 6		15·8	15·3	12·98

and considering the sources of error that came in, I think they indicate that the results will be the same for all distances—but I must clear the box of Cobwebs and then go over them again.

12499. First thing. The beam and inside of the box and even the torsion wire were well brushed over with fine feather and down brushes—to clear away all filaments and webs. Then the beam was set at 0° and the telescope at 6. The beam vibrated well and freely when the torsion was 0°, as if all clear.

12500. The general temperature was 63° F.

12501. Distance 0·5, and No. 6 (12035) in air—its force 46°·5. In water, the torsion was made 4°·5, place being at first 6·4—then 6·7; made the torsion 5° with place 6·3—6·2. This made torsion 5°·6 with place 5·9—5·85. Consider the torsion as 5°·3 in water. Then the place of No. 6 is 12·8 below water.

12502. Distance 0·4 and *No.* 6—in water—torsion 9° with place 5·9—5·8—steady. Take the torsion a little less for 6—and at 8°·8. In air, the torsion force was 76°·5. So these results give the place of No. 6 as 13 below water.

12503. Distance 0·4 and No. 2 (12035); in air, 126·5. In water, was 12°·5 with place 6·1—13° at place 6·08—left whilst I dined for 30 minutes, and then was at place 6·02. Take 13°·2 as the torsion. So the place of No. 2 is thus 11·65 below water (11·2, see below (12505)).

12504. Distance 0·4 and No. 7 (12035) in water—torsion 11°·6 at place 6—6·06—6·06. Take it 11°·7 in water. Then in air—i[t]s torsion was 112°·4. These numbers give 11·6 as the place below water of No. 7 (or 11·1, see below (12505)).

12505. Verified the torsion; it is about 0·5 out—to be taken off direct torsion. Assuming this to have come on with the heavy weights and greater torsion of the last two expts. (12503, 4), it would correct their numbers to No. 2 at 11·2 and No. 7 at 11·1.

12506. Distance 0·3. No. 6 in air was 116° of torsion. In water was 10°·6 at place 6·05—6·09—15 minutes after was 6·2. So make the torsion for water 10°·8. These numbers give No. 6 as 10·26 below water.

12507. Distance 0·7. No. 6 in water. Very low forces, torsion 4°·2 at place 5·8—5·15—4·8—4·6—4·4—4·3—4·2. Now I had opened the room door and the screens were not close at the magnet, and this very regular sinking made me think there was some effect other than a mere feeble current in the fluid. It went down to

place 4·16–4·1. I then lowered the torsion to 3°·5, when the place first rose, then fell, being successively 4·5–5–5·1–5–4·95–4·9. Then lowered the torsion to 3°–the place became 5·3–5·5–5·7–5·8–5·75. Lowered the torsion to 2°·6–place 6–6·2–6·3–6·3–6·25–6·23–6·2. As the torsion is not quite enough here, may take it as 2°·7 in water. Is almost at Zero, and so gives very slow motions and obeys any motion in the fluid.

12508. At one time I thought that change of temperature by cooling from the opening of the door was doing something–then that it might be a slow undoing of a former set, as from (12503, 4), but on the whole incline to believe it was the slow movements of the fluid.

12509. Now removed No. 6–took a film from off the water by paper (   ), restored No. 6, and found that with the 2°·6 of torsion left from the former experiment–the place was first 12, then 11–10–9–8–7–6–5–4·99–5·1–5·2–moves slowly–now 5·3–5·5–5·67–5·75. As the motion was exceeding slow, made the torsion 2°–and the place rose soon to 6–6·2–6·3–6·4–6·5. So assumed 2°·5 as the true torsion in water.

12510. Now No. 6 in air at this distance of 0·7–made the torsion successively 11°–11°·5–12°–12°·6–12°·9–13°·5–between 13°·5 or 13°·7 near the truth.

12511. Verified the torsion–was out about 0·25 to be taken off direct torsion–

So No. 6	in air	13°·6 or	13·35
	in water	2·5	2·25

giving the place of No. 6 as 22·5 or 20·2. So one sees what large errors creep in when the object is so near the fluid, and when the interval in degrees between air and water is small–as at this great distance.

12512. The Air 13°·6 would require Water 1°·6–or the water 2°·5 would require air 21°·7, to give 13 as the result below water.

12513. Made the Distance 0·5. No. 31 pure Zinc (12035)–in Air, 54° or 54°·5. In water–repelled, but very feebly; is too near Zero. When torsion 1°·5, was at place 6–went to tea–found it afterwds. at place 7–so 1°·5 very near–perhaps torsion 1°·6 better for Water. This gives the place of No. 31 as 3·05 below water.

12514. Made the distance 0·4. Now No. 31 in water had torsion

made gradually $3^\circ-4^\circ-4^\circ{\cdot}7$—and with the $4^\circ{\cdot}7$ the place was successively 6·4—6—5·9—5·84. I think a torsion of $4^\circ{\cdot}5$ will be about right for water. In Air at this distance of 0·4, the torsion was $83^\circ{\cdot}5$. These data give the place of No. 31 as 5·69 below water—and because the difference between it and water is greater, being $4^\circ{\cdot}5$ in place of 1·6, I think the latter like to be the better number of the two for No. 31.

12515. This pure Zinc No. 31 was very variable by the Electro magnet (12217, 462). By the Great Logeman it was before (12428, 30) 10·4 *below water*, at the distance of 0·5 (12430) and 7·14 *above water* at the distance of 0·7 (12475). Rejecting the Electro magnet results, those with the Great Logeman are as follows: at

0·7 distance	7·14 above water		(12475)
0·5 „	10·4 below water		(12430)
0·5 „	3·05 „	„	(12513)
0·4 „	5·69 „	„	(12514)

The variations can hardly be the effect of distance variation—the two upper ones are subject to the idea of spider's web. Must repeat No. 31 at different distance.

12516. Supposing that to-day's results may be trusted, and the former not, because of currents, Web, etc., then those with No. 6 are at increasing distances as follows:

distance of 0·3	10·26 below water
0·4	13
0·5	12·8
0·7	20·2

So that the glass No. 6 appears to be lower in relation to water for greater distances than for small—but the last result of 20·2 can hardly be trusted, because of the little difference from water, and must try the effects at 0·3, 0·4 and 0·5 and even 0·7 again.

12517. Taking No. 7, which is the same glass as No. 6 but twice the length, it gives to-day

	11·1 below water for distance 0·4 and on			
former days	11·4	„	„	0·3
	11·1 and 10·85	„	„	0·5
	11·2	„	„	0·7

So that here there appears no variation in the result by distance,

and what is more, the mean 11·2 is not far from 12, the mean of the results for No. 6 at 0·3, 0·4 and 0·5 distance.

12518. No. 2, which gave the following series (12498):

12·1	below water	for dist.	0·3
17·8	"	"	0·4
11·6	"	"	0·5
11·3	"	"	0·7

having had a result (more safe) to-day for the 0·4 distance, gave 11·2 ( ) instead of 17·8, and this number accords closely with the former numbers for other distances.

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12519. Brushed out the box and beam. Adjusted Zero–telescope place, etc. Temperature 60° F.

12520. The balanced light beam, being left, slowly moved round with the object end receeding from the magnet, as if a little direct set were being pulled out or going back again now the weight of the beam is on the wire–the wire image in the telescope was successively 6–6·2–6·4–6·5–6·7. Loaded the beam heavily to pull out this state and the effect still went on, and after an hour had amounted to 2°.

12521. It seems that when the wire is stretched, it gradually gives in this direction, and that when relaxed, as since the last working day, it gives in the other direction. Perhaps the spiral condition of the wire may have something to do with it. The suspender should be of glass, but that is not strong enough for my purposes.

12522. If this effect, or rather the return effect, or either, comes on quickly, I must not observe when objects are *near water* in the force, for then the changes would tell easily, and tell much in relation to the place as regards water.

12523. I will leave the beam with the weight upon the wire in some degree at night–perhaps the effect will go out or disappear. The effect is very important as regards verification of Zero. Is it possible that it might be any electrical effect, for I had just been wiping out the box and beam with a feather brush and the beam is flint glass? It might be wise to wipe out the box when about to leave it rather than when about to use it.

12524. After this, observed the place of the beam when heavily loaded or lightly loaded, the torsion index remaining unmoved. With the heavy beam, the extreme vibrations were as follows:

3·3		
	5·1 mean	
6·9		
	5·3 ,,	
3·7		
	5·2 ,,	general mean, 5·15.
6·7		
	5·4 ,,	
4·1		
	4·75 ,,	
5·4		

So that the place in the telescope was 5·15, or may be taken so. Then with the beam light, the numbers were as follows:

2·1		
	2·85 mean	
3·6		
	2·9 ,,	
2·2		
	2·87 ,,	general mean, 2·85;
3·45		
	2·9 ,,	
2·35		
	2·75 ,,	
3·2		

and the place only 2·85, which is 2·30 different from the former or heavy beam result. Then loaded the beam again and found the places as given and the general mean or place 5·41.

10		
	5·55	
1·1		
	5·15	
9·2		
	5·6	
2		general mean, 5·41.
	5·27	
8·54		
	5·56	
2·58		
	5·32	
8·05		

After leaving it a good while, found the places to be thus:

4·5
    5·8
7·1
    6·34 } general mean, 6·1,
5·58
    6·17
6·76

and the place of the mean as 6·1. As if the effect before spoken of (12520) were still going on with this heavy charge. In any case it is seen however that the place of a light beam is not that of a heavy one, that the difference is from 2·3 to 3·25, which is from 1° to 1°·5 of torsion. Now the higher numbers are farther out than the lower, and therefore to alter the higher to the lower would require a *direct* motion of the torsion plug to the amount of 1° or 1°·5; i.e., if the torsion index is left from an unloaded beam, being then at Zero, and the beam is heavily charged and employed, the direct torsion must be diminished in the latter case by a quantity proportional to the difference.

12525. Thought of putting in a new platinum wire straightened by heat and stretched to make it hard. Took a piece of the same wire 18·75 inches long—heated it in the spirit lamp whilst held out straight—when cold, broke very easily by pulling, and did not stretch much, and I doubt whether I should do much good by such a process—perhaps much harm because of the softness of the heated wire. Better go on at present adjusting the beam to place 6 in telescope when light, and keeping a record for change of Zero for a heavy charge. So made the beam light, and finding

1·65
    2·7
3·75
    2·95
2·15
    2·75
3·4
    2·9 } general mean 2·86
2·40
    2·85
3·32
    2·95
2·59
    2·92
3·25

the places as given and the mean 2·86, which is almost the same as the former, 2·85 (12524), I now for the state of things made the Index at 0°.

12525½. I now found that setting the ring scale to the torsion index, because it tended to give a twist to the box top and the whole frame, caused an alteration in the apparent place of the beam. Thus the top has just had a push *reversely*, and the places in the telescope are 5·4–6·7–5·6–6·48–5·7–6·3, giving the following means, 6·05–6·15–6·04–6·09–6, and the general mean of 6·06. Then giving a direct push or twist to the top of the box, the places came out 5·24–6·8–5·05–6·42–5·20–6·21–5·4–6·17–5·58, the successive means being 6·02–5·92–5·73–5·81–5·70–5·8–5·78–5·87, and the final mean 5·83, which is 0·23 less than the former mean or place. This is not quite the eighth of a degree of torsion–still, it is something and teases one.

12526. Left it for 23 minutes, and returning found the beam almost stationary, but the place had been rising–the extremes of vibration were 6·63–6·52–6·44, and the means 6·57–6·46–just as if the first rising effect (12520, 24) was still going on–over all the other changes.

12527. Leaving the plug and wire quite untouched, I now pushed the circular scale on the top of the box round 5° *reverse*. The places of extreme vibration came out 6·4–6·6–6·25–6·36–6·18–6·34–the means are 6·5–6·42–6·3–6·27–6·26, and the final mean 6·35 being the place. Now pushed the ring index 10° direct, i.e. 5° past Zero direct. The extreme vibrations were 6·68–6·11–6·39–6·06–6·32–6·1. The successive means are 6·39–6·25–6·22–6·19–6·21, and the final mean 6·25 being the place. This is only 0·1 of difference, or the $\frac{1}{20}$ of a degree. As before, the direct push gives the lower numbers (12525½).

12528. I opened the room door a while but did not find any change by that.

12529. It will be better not to touch the balance scale ring, but either leave it at Zero when 6 is in the telescope, and then either note the variation in the telescope and make that the correction–or set at proper times the telescope scale to 6, making the change there rather than at the ring, unless the quantity be large. So now turned the ring back the 5° (12527), making it at 0° to the index and finally left the telescope [at] place 6, and began to observe.

12530. The distance has all this time been at 0·4—made it 0·3—and then observed the vibrations given by the shake to see if moving the box had affected the place. The beam had been taken up on the fork during the motion and was now let down again—the numbers were 2·8—9·4—3·65—8·25—4·15—7·43—4·5. The different means are 6·1—6·52—5·95—6·2—5·79—5·96, and the final mean 6·04. So that moving the box had not altered the telescope place sensibly, for the difference ·04 would probably have disappeared under longer observation.

12531. After an interval of not more than 2 minutes, looked, expecting to find the beam vibrating about 6—and found it instead almost still and vibrating at 5·5—5·6. The bob put at 4 to keep the scale place was still there and I do not know how these 0·5 or 0·4 have been lost. Might have been a touch upon the telescope—perhaps was—but I am not aware of it. However, as all seemed at rest nearly—adjusted to place 6—when the vibration of the beam not more than 0·1 in the telescope.

12532. Began to observe with No. 6 glass cylinder (12035) at increasing distances. First *No. 6 and distance* 0·3. In air, 227°. In water, successively 19°—20°·7—then place successively 6·05—6·05—6·02—make the force 20°·8. So in air, 227°—in water 20°·8, giving the place of No. 6 as 10·08 below water.

12533. Made distance 0·4. Then No. 6 in water (for it had been left in during the motion of the box)—gave 12°—place 5·82—11°·5 and then place 5·95—5·92—so made place in water 11°·7.

12534. Thought it possible that in wiping the object when it came out of water, it might be left electrical if not especially attended to. I damped a clean cloth and now wiped it with that—and intend to do so in all cases. It was impossible that the glass object could now be electrically excited. In air was 115°·1. So in air, 115°·1—in water, 11°·7—and the place by the data of No. 6 is 11·3 below water.

12535. No. 6 and distance 0·5. In air, 66° torsion—place 5·08—4·89—5·07—4·89—make number 65°·8. (I copy the pencil notes accurately but I cannot think what I mean by these places, unless I have made a mistake in writing and that they should be 6·08—5·89—6·07—5·89). The pencil notes then give:

in Water, 7°·2—place 6·08—6·02—settles force at 7°·2

in Air 64°–65°. Now 65° air and 7°·2 water gives 12·4 as the place of No. 6 below water, and 65·8 and 7·2 gives 12·1 as the place.

12536. No. 6 and distance of 0·6. In air, 42°·4 torsion for places close to 6 or about it, but corrected gradually to 41°·5. In water, first 4°—then 5° with places 6·2—6—5·9. Made 4°·8 torsion—gave places 6·3—6·08. Settled on torsion of 4°·8 or 4°·85. These numbers give 13 or 13·2 as the place of No. 6 below water.

12536½. At this distance the vibrations very slow and large.

12537. No. 6 and distance of 0·7. In water, as left from last experiment, torsion was 3°—3°·5—3°·7. The object creeps up to its place with extreme slowness.

In Air, 27°·6. So the place by these figures, 15·4.

12538. Supposing air to remain 27·6. If water were 3°·2 instead of 3°·7—it would give place of No. 6 as 13 below water—or if water were 2°·8 it would give 11·3.

12539. Now the object was taken off, but weight put on the beam above to equal that of the object, so that the *true place* of the beam with the weight might be obtained. Its vibrations were then 3·3—10·9—4·4—10·1—5·35—9·6—the successive means are 7·1—7·65—7·25—7·72—7·47—and the final mean 7·44, for the place of the beam thus loaded and free from the Magnet. Now 7·4 farther out from the magnet than 6 by about 0·7 of torsion scale, i.e. the Zero is back 0°·7, and direct torsion ought to be diminished by that quantity. This is very important for the latter numbers which are so near water. The difference is accordant with that before found to belong to a heavy beam (12524).

12540. Now the numbers obtained are as in the first column of results:

			diminished by 0·7 gives		as the place below water instead of
At 0·3 distance in	Air	227°	226°·3	9·074	10·08
	Water	20°·8	20°·1		
0·4 " "	Air	115°·1	114°·4	10·64	11·3
	Water	11°·7	11°		
0·5 " "	Air	65°·8	65°·1	11·1	12·1
	Water	7°·2	6°·5		
0·6 " "	Air	41°·5	40°·8	11·17	13
	Water	4°·8	4°·1		
0·7 " "	Air	27°·6	26°·9	12·1	15·4
	Water	3°·7	3°		

12541. Both columns of places seem to shew a lower place for glass as the distance is greater or the force less, and though the last column no doubt wrong, yet the corrected one requires consideration; and unless it can be referred to a change progressing through the whole, or to the result of difficult settling in the water, I do not see what it can be except a true difference of magnetic action due to the change of capacity with different forces; i.e., a change the progression of which is not the same for Glass and water. It can hardly be the change of 0·7 progressing through the whole, because if no correction be made for the first two or three numbers, i.e. for results at 0·3 and 0·4, still these results are below those for 0·6 and 0·7 distances corrected. I should have to diminish the results at 0·7 by 1°·3 of torsion both for Air and water to make the place *equal* to that given by the results at 0·3 uncorrected.

12542. If we suppose that for 0·7 distance the air is right as 26°·9, and see how much water would require to give about 10 or 10·2 as the place, then the force in water must be made 2°·5 instead of 3°. Now half a degree is very uncertain; still a very little change of force changes the place in the telescope at these low degrees, and I think the effect of half a degree could hardly escape me.

12543. I must carry No. 6 through the series from great to small distances, and then see what comes of it, and whether the order is the same or reversed. If there be a difference in capacity effect, it will no doubt come out better in some other cases, i.e. with other objects than glass.

12544. When the beam was at place 9·3 from former experiment (12539) and still swinging, I merely stopped the swing by the fork at 6·9 and let it loose again, and then it went to 9 and next to 7·1–8·7–7·4, as if it had suddenly settled and given a new place 8; and in fact this was so, and I had to alter the telescope scale. I made the place 6 whilst thus moderately loaded.

12545. There appear to be sudden settlings of the wire apparently, at its grip below perhaps (12531).

12546. In regard to a polarity slowly assumed or lost, or any state correspondant to such a notion. I made the distance permanently 0·7 and put on 3°·5 of direct torsion by the index, which

was also left unchanged for the following experiments. No. 6 Glass cylinder, was held close to the magnet poles in the water—put on to the beam—and its final place of rest observed, starting from place 11—it moved rather freely and gradually moved up by 10—9—8—7—6—5—4, more slowly to 3—then after a while very slowly retreated to 3·2—3·3—3·4. As this was closer than I wanted it, I made the torsion at the index 3°, and now left it whilst I went to tea. Half an hour after, I found the object at place 4·6, and quite still—will accept that as the place of the No. 6 under this torsion and conditions.

12547. Took off Object, turned it 180°—put it quickly on the beam again—set that free; the object went out by the impulse of the fork and then returned slowly, creeping up to 4·7—it then receeded a little very slowly to 4·76 and finally to 4·75. Again raised the fork to lift the beam a little—let it down again—the object again went out by the impulse and slowly returned to 4·7. Again raised and lowered it—sent out but not so much as before, only to 8—and returned to 4·7 and even to 4·35.

12548. I could see no signs here of any effect due to the reversion of the glass—any thing indicating that it could take up and keep a state so as to affect the result by it, though I repeated the attempt to produce reverse effects.

12549. Experimented with No. 31 (pure Zinc) (12035), in reference to its true place in respect of water (12475). Made the distance 0·7 and the torsion 0° by the index. Was in water. Went out at first and stood out long, as if repelled, but found this was only the result of an impulse from the fork in letting the beam down. For by degrees the Zinc crept up toward the magnet, and at last, though very slowly, came to place 6, as if it were neither magnetic nor diamagnetic in relation to water. It gradually crept on to 5·7—5·6—5·55—then out again to 5·6—5·62. It is not sensibly attracted or repelled at this distance.

12550. Altered nothing but the distance, making that 0·6 by the use of the box screws. The distance in the telescope appeared to be 8·1. This was a consequence of the fact that when the whole box and beam was carried 0·1 nearer the magnet, the zinc in the water was held back by it, the center of suspension advanced about the 0·1 and the outer or counterpoise end would advance

0·2 or more, giving the telescope difference. Waiting—the Zinc gradually went in to 7·8—7·75—7·6—7·5, there being no torsion on except that caused by the deflection of the beam itself, and this was enough to carry the zinc in towds. the magnet, so that the zinc cannot be far from water, if at all so.

12551. Did not wait, but made the distance 0·5. The same effect as before, for I found the beam at place 9·5, but the numbers increased to 10—10·4—10·48—and then began to diminish again to 10·3—10·1—10—9·9—9·8—9·7—9·5. The first increase after 9·5 was a consequence of the momentum due to the advance of the counterpoise end (12550), but that being over at 10·48, then the torsion due to deflection carried the beam back towards its normal position.

12552. Made the distance 0·4. Then in the telescope the place was 11·5—11·7—11·8—12—and then back to 11·9—11·8—11·7—11·5—11·3— — —10·5— — —10·42. It is now nearly at rest and the effect shews that the zinc is a trace diamagnetic in or to water. Will examine what the mere state of wire and torsion without the Zinc will do presently.

12553. But first the Zinc, being now at 0·4 distance, and its telescope place 10·42. Took it off the beam, turned it on a vertical axis 180°, put it on again, set it free and looked for its place in the telescope. It had gone in to 6 by the accidental motion and now went out slowly to 7—8—8·6—8·74, etc., and would if I had waited crept out further. So that there was no sign of a *polarized or equivalent state*—of any state indeed acquired by vicinity to the magnet, which could affect its place, for here in water where it was so easily displaced by any difference of power, the difference of place is only 1·7, and by waiting a little longer would probably have become nothing (12475).

12554. Took off the Zinc, No. 31, and put on equal weight on the beam above, the torsion not having been touched; and now the vibrations were 11·3—1·7—10·1—2·5—9·4—3·2—8·6—3·8—the successive means being 6·5—5·9—6·3—5·95—6·3—5·9—6·2—and the final mean 6·15. So the beam and wire in very good state, and the difference between 6·15 and the number in water will shew the state of Zinc and degree below water. In water, by falling it came to 10·42 in the telescope—in rising it came to 8·74—

and the mean can not be far from its true place 9·58. The difference between this and the beam number 6·15 is 3·43 inches on the telescope scale, equivalent to $1^{\circ}\cdot71$ of torsion at that distance of 0·4. Now a former result at 0·4 gave the force in air $83^{\circ}\cdot5$ (12514)—so that the data at present would appear to be: in air 83·5, in water $1^{\circ}\cdot71$, and its place below water 2·09. This however is not quite correct, because in fact the $1^{\circ}\cdot71$ of torsion are given by the Object being pushed out so far from the Magnet, and not by the motion of the index, and one ought to see what torsion at the Index would reduce the telescopic place 9·58 to 6·15 at this distance of 0·4, or with this Object.

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12555. Beam appears right at first sight. Torsion plug at 0°. Temperature 61° F.

12556. No. 6, weight of 43 grains; made a lead weight equal to it and put it on the object end of beam, so that by changing these, whether the object was on at the magnet or only its equivalent on above, the weight on the suspension wire should be the same. Balanced the beam with this on and adjusted the telescope to place 6. Then moved the box from 0·4 to 0·7 distance by the screws, and found another sudden settlement of the torsion. I think these must occur at the attachment below and must have that altered. Found the place in telescope falling slowly to lower numbers.

12557. Brushed out the beam and box—reballanced the beam and left it to itself—observing its place, found as the extremes of

6·25 6·1 6·48 6·38 6·65 6·54 6·75 6·63 6·75 6·75

vibration: 8·4—4·1—8·1—4·86—7·9—5·4—7·68—5·82—7·44—6·07—

6·91 6·9 7·03

7·44—6·38—7·42—6·64—so that it seems to be rising. After 46 minutes from the last observation, it was 8·18, and in 50 minutes more, still 8·18. So the settling over for the present. Made this place 6 in the telescope by adjustment of the scale.

12558. At 1 o'clk., altered the torsion index and the Zero (direct) about 2°, to set the beam right in the box for Magnet distance, leaving the telescope unaltered. At $1^{h\cdot}$ $30^{m\cdot}$, found the telescope place to be 0·8—1·25—i.e. at 1, so has changed 5 inches. I found the telescope scale was *69 inches* from the reflector ( ), and

a thirtieth of this gives 2·3 inches on the scale to a degree in the telescope. So these quantities correspond very well. Made the place 1, place 6 in the telescope, and proceeded to work with No. 6 object, always being careful to wipe it with a damp cloth when taking it out of water.

12559. No. 6 at distance 0·7, in *air*, 25°·8. In water, 2°·7–place successively 6·15–6·1–6·1–torsion 2°·72. Hence No. 6 appears to be 11·78 below water.

12560. No. 6 at distance 0·6; in Water, 4°·8–in Air, 40°–and its place 13·6 below water. I think the torsion in water perhaps a little too high.

12561. No. 6 at distance 0·5; in air, 65·7–in water, torsion 7°–place 5·7–5·75–5·82–5·85–torsion 6°·9. The place 11·73.

12562. No. 6 at distance 0·4; in water, torsion 12°. Place 6·3–6·2–6·15–6·11–6·08–6·06–torsion 12° or 11°·9. In air 118°·3.

12563. Through forgetfulness, I again put No. 6 into water at this distance of 0·4; torsion 10°·9–place 5·89–5·92–5·98–6–so torsion 10°·9. Now this differs a full degree from the former and I do not know why. The place of No. 6 is with water 11°·9, at 11·18 below water–and with water 10°·9 is 10·15.

12564. No. 6 at distance 0·3; in water, 22°·6–place 5·8–5·85–5·9–torsion 22°·5. In air, 241°. I thought I saw the place changing a little here, as if the torsion were losing, though that is hardly likely to come visible so quickly. Must try it. As it is, the place comes out 10·3 below water.

12565. Now the object was taken off but its equivalent placed on the beam and the beam left to give its telescope place–successive vibrations were 8·9–2·5–7·93–3·3–7·4–3·94–7·0– – –4·7–6·4–etc. The successive means are 5·7–5·21–5·61–5·35–5·67–5·47– – –5·5–and the final mean 5·5, so that 0·5 or about $\frac{1}{5}$ of a degree only has altered, and that during four successive alterations of the place of the box.

12566. The numbers obtained to-day give the following places for No. 6

at the different distances of	0·3	0·4	0·5	0·6	0·7
No. 6,	10·3	11·18 or 10·15	11·73	13·6	11·78
No. 6 before (12540),	9·074	10·64	11·1	11·17	12·1

The place of the glass seems to be lower in relation to water as the distance is greater, as before (12541) but nothing like to the same amount; for the effect at 0·6 (12560) cannot be depended upon. The point will require investigating with some other substance. As the distances were now made less and less whilst before they were made more and more, the effect if any does not depend on the direction in which the experiments are made.

12567. No. 31, Pure Zinc (12554) in water at 0·6 distance–its telescope place was 6·6–put the torsion plug about 1°·3 back and then the place became 9·45, or 2·85 difference.

12568. A specimen of the torsion wire sustained 4850 grains, and then broke with 500 gr. more, near the wax which held it. Again it underwent and sustained the same trial. Having 4 wires ready for use, I have hung a weight of about 2000 to the end of each and left them to stretch, etc. from this time onwards.

12569. Have taken down the beam and sent it to Newman to have the mode of attachment of the wire more accurately and solidly made.

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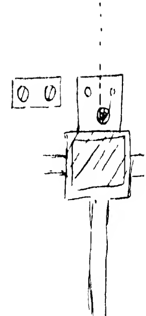
12570. Weighed parts of the beam. The glass beam itself = 128 grains–the box carrying it and attached reflectors with the new mode of attachment of the wire, 324 grains. Three ballasting cylinders of copper to screw into the bottom of the box weighed respectively: No. I, 125 grains; No. II, 95 grains; No. III, 67 grains.



12571. The attachment for the wire below is now effected first by a screw in the upright plate below, round which the wire passes, and is then griped by the head of the screw against the plate. The wire then lies in a very shallow scratch in the upper part of the plate and is nipped by a second plate which goes over the wire and screws on to the first plate, and is alone able to hold it.

12572. Put in a new torsion wire, one of those stretched since the 21st by the weight (12568). All seemed well. Employed the No. II ballast cylinder = 95 grains, so that the beam, box and ballast cylinder altogether weighed nearly 550 grains.

12572[1]. Balanced the beam light and adjusted the torsion scale, etc. until the beam was parallel to the sides of the box and the index

[1] 12572 is repeated in the manuscript.

at 0°. Found by a few observations at the telescope the successive places as 6·23–5·69–6·28–5·86–6·22–5·81–6·04–5·8–the successive means are 5·96–5·98–6·07–6·04–6·01–5·92–5·92–and the final mean is 5·98, which may be considered at present as 6, or the constant place.

12573. Then loaded the beam heavily with 530 grains more and left it to vibrate. I had now to alter the torsion Zero 2°–did so and made zero at 0°. Now the successive places in the telescope–
6·1 6·0 6·32 6·22 6·41 6·28 6·44
and their means were 8·5–3·0–8·3–4·34–8·1–4·72–7·84–
6·33
5·04–7·62–gradually getting to a higher number and by two series, as the means occur after a left hand or a right hand observation, as $^{6\cdot1}{}_{6\cdot0}{}^{6\cdot32}{}_{6\cdot22}{}^{6\cdot41}{}_{6\cdot28}{}^{6\cdot44}{}_{6\cdot33}$. The whole was left for 55 minutes more, and then the place was found to be 7·2 or 7·1–having gradually risen to this point; the vibrations were now of exceedingly small extent.

12574. At this time I made the beam light again to see what effect that change would produce. On first observing, the posi-
6·3 5·4 6·1 5·45 6·05 5·75 6·2 5·91 6·22 6·03
tions were 12·1–0·6–10·2–2·1–8·8–3·3–8·2–4·2–7·62–4·82–
6·26
7·24– – –and after 18 minutes more the positions were 6·33–
6·26 6·27
6·2–6·32–6·22. So 6·26 is the place now, and thus the light beam is 0·9 lower than the heavy beam was–but we do not know that the gradual change is over. Still, a light beam has a lower place on the telescope scale than a heavy one, by an effect dependant on the stretch of the wire from different weights.

12575. Put on the beam the equivalent of No. 6 object and also the counterpoise (12556)–balanced and left it; after a time observed the places thus–7·76–6·2–7·7–6·7, but found here that the card boards at the magnet were a little open and might admit
7·5
of small currents–shut them well and observed again–7·16–
7·72 7·66 7·76 7·77 7·83 7·8
7·84–7·4–7·92–7·6–7·95–7·72– – –7·86–7·75. So the present place is 7·8, the weight having brought it up from 6·26 (12574) towards and above 7·1 (12573). I left it thus, to hang with this full weight on the wire, from this time Saturday 7 P.M. until

next Tuesday, marking the 4 place of the telescope scale by a bob, and also observing that the counterpoise end of the beam was over the ⊥ marked inch of the scale in the box. It may by monday have got out of the telescope.

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12576. The beam has been left hanging 63 hours—found it and the balance, etc. generally right, but it has gone reverse direction and now its place in the telescope is 8·82—it was left at 7·8 (12575).

12577. The telescope or its scale had not shifted.

12578. Put a small wooden plug in at the top of the torsion plug to hold the wire firm there. Brushed out the box and the beam. Put on the No. 6 weights (12556). Adjusted the torsion plug to its proper Zero and the telescope scale to its place of 6. Altered the box distance from 0·5 to 0·7, and after that found telescope place was still 6 or 5·99.

12579. Put more weight on the beam to equal that of Object No. 7 and its necessary counterpoises: found the place in the telescope altered to 4·66, as if more weight lowered the number and made the place nearer the magnet, the wire unwinding as it were direct. Adjusted the scale with this weight to place 6.

12580. Now began to experiment with Object No. 7 (12035), being the same glass as No. 6 but as a cylinder twice as long and as heavy, and extending a good way both above and below the magnetic poles. This was tried at several distances, beginning at the greatest when there was the least torsion on the wire and proceeding to the least when there was most. The object was always repelled both in air and water.

12581. No. 7 at distance of 0·7—in *air*, 29°·8 direct torsion—in *water*, with 2°·9 torsion, places successively 5·6—5·8—5·9—6—6·1—so torsion too little—made it 3°—then place became 6·05—with torsion of 3°·4, place was 5°·95. Believe it was 3°·3 or 3°·4. These data give the place of No. 7 as 12·45 (or 12·87) below water.

12582. No. 7 at distance 0·6—*in Water*. With torsion 6°—went past place 6 on to 5·8, etc. Made torsion 5°—object went out to 6—6·25. Believe 5°·1 is the torsion or between that and 5°·2. In Air—the torsion was 40°·4. Hence the place is 14·4 below water.

12583. No. 7 at distance 0·5. In *air*, torsion 61°. In *water*, with

torsion 7°·5 – too much – made 7°, nearly right – left for 30 minutes and then the place 6·08. So take the true torsion as 7°·1 or 7°·2. Then the place of No. 7 is 13·17 (or 13·3) below water.

12584. No. 7 at distance 0·4. In *water*, torsion 10°·1. In air, the torsion 95°. So place of No. 7 – 11·9 below water.

12585. No. 7 at distance 0·3. In *air*, torsion 167° (166°·7)[1]. In water – torsion successively 16° – 18° – 17°·4 – 17°·3. Decide on 17°·3 (17°·0) as correct by the experiment. So place of No. 7 thus is 11·55 (11·35) below water.

12586. No. 7 at distance 0·2. In water – successively torsion of 25° – 30° – 32° – 31°·3 (30°·2) – settle at the last. In air – 320° (318°·9). Place of No. 7 is 10·84 (10·46) below water.

12587. Now 320° is a high torsion on the wire and may have given a set – but still I went on to two other distances backwards, increasing the distance thus.

12588. No. 7 at distance 0·5. In air – torsion 66° (64°·9). In water, the torsion was 8° (6°·9). The place thus 13·79 (11·9).

12589. No. 7 at distance 0·7. In *water* – torsion 3°·5 – 4°·5 – not enough – 6°·0 too much – 5°·5 – 5°·6 (4°·5) or between that and 5°·5. (After some experiments to be immediately related (12590, 1, 2) the torsion in *air* was obtained as 30°·4 (29°·3) which with water 5°·6 (4°·5) gives the place of No. 7 as 22·58 (18·1) below water).

12590. Now this result of torsion 5°·6 (4°·5) in water at distance 0·7 is so different from the former torsion of 3°·3 for the same distance (12581), that I could hardly think it due to a set in the wire from the large torsion of the 0·2 distance (12586), and suspected that the glass had acquired a polarity or state either by *vicinity* or *time*; for during all the experiments it has been put on to the beam in the same position and therefore has continually been subject to the magnetic power in the same direction. To try this notion, the distance and torsion and water cell and all other things were left unchanged – but the object No. 7 was taken off – turned through 180° to reverse its position in relation to the lines of force – replaced; and its telescopic place observed. The mechanical action had thrown it out, place 8 or more, from whence it went up regularly nearer and nearer to the magnet through

[1] Results in brackets in pars. 12585–12596 are in red in MS. (see par. 12595).

places 7·5 – 7·0 – 6·5 – 6 – 5·9 – 5·8 – 5·7 – 5·6 – 5·5 – 5·4 – 5·35 – 5·3, moving slowly now as if near its place of rest – 5·25 – and after much time still dimd. the distance to 5·23.

12591. So now there is clearly too much direct torsion on, for the 5°·6 (4·5) which before the reversion held the object at telescopic distance of 6, now is able to drive it nearer and up to 5·23; as if the glass was more diamagnetic in the first position than in the second, and as if the glass had been made more diamagnetic than it was at the beginning by continual subjection in one direction to the magnetic power. Now if that were the case, then this reversion of its position should have reduced this acquired extra diamagnetic power in some degree, and indeed, unless much time was required, reduced it altogether. So took off the Object No. 7, turned it 180° and put it on the beam in its first position. Its place by mechanical force was at 10, from which it passed to 9 – 8 – 7 – 6·5 – 6 – and *past* 6 up to 5·9 – 5·8 – 5·7 – 5·6 – 5·5 – 5·4 – 5·3 – 5·2 – 5·15 – and after a long time it stood at 5·1.

12592. So it does appear to have lost the state it assumed during the experiments, and now seems to be alike for either position, having place 5·1 in the last position and place 5·23 for the other one (12591, 12590). I had to diminish the torsion to 4°·5 (3°·4) in order to have a present place of 6 in the telescope. This will require a further correction before the true torsion is obtained of the glass in water at present (12595), but this correction will not affect the general result of change in position (12590, 1) just described.

12593. It was now that I ascertained with this turned and returned object No. 7, its torsion in air at the distance 0·7 (   ) and found it to be 30°·4 (29°·3) as said (12589).

12594. I now tried No. 7 in water again at this distance of 0·7, after holding it in the water close up to the magnet for a moment and then *reversing* it (through 180°) at a distance, and so putting it on the beam with a torsion of 4° (2°·9) at the index. When first observed in the telescope, its place was 9 – then successively 8·5 – 8 – moving very slowly – 7·5 – 7 – 6·5 – 6·0 – 5·9 – 5·8 – so that at this moment the torsion too much. Made it 3°·8 (2°·7) and then the place increased to 6 – 6·1 – 6·15. So that the experimental torsion was 4° (2°·9) or 3°·9 (2°·8) very nearly. And as this is

less than 4°·5 (3°·4) (12592), the reversion directly before the experiment and measurement appears to [have] had an effect and diminished the power of the glass as a diamagnetic in this position. (See further results, 12625, 12636, etc.).

12595. Finally, to obtain the effect of any set given by the high torsion of distance 0·2 (12586), took off the object and replaced the weights used at the commencement when index Zero was adjusted to telescope place 6 (12579). The successive places of

8·1 8·8 8·5 8·97 8·67

the vibrations, being watched, were 3·8 12·5 5·1 11·9 6·05

9·02 8·82 9·1 8·9 8·84 8·97

11·3 6·75 10·9 7·3 10·5— — —8 9·68 8·26. So that 8·95 is the present place. Now this is 2·95 more than 6, and 2·95 equals 1°·1 degrees of torsion ( ) change. As the beam stands out farther by this much, the Zero ought to be where 1°·1 of direct torsion is, i.e. the direct torsion has to be diminished by this quantity at and since the expt. at 0·2 (12586), for I have no doubt this set was chiefly given there—and I now make *this correction at the different places in red ink.* It brings the last expression at 0·7 distance of 4°·5 to 3°·4, and makes it identical with that of the first experiment made at that distance (12581).

12596. The place of No. 7 below water at the given distances is as below, both for the original and the corrected results—and in the order observed.

At distance of	0·7	12·45	(12581)
	0·6	14·4	(12582)
	0·5	13·17	(12583)
	0·4	11·9	(12584)
	0·3	11·55 (11·35)	(12585, 95)
	0·2	10·84 (10·46)	(12586, 95)
	0·5	13·79 (11·9)	(12588, 95)
	0·7	22·58 (18·1)	(12589, 95)

12597. Now here as before (12566), the place of the glass is lower in relation to the water as the distance is greater; i.e., the glass as a diamagnetic surpasses water more at greater distances, and probably with weaker powers, than at less distances and higher magnetic power, and that is the case both in the first and the last results, though the two first are irregular.

12598. In the next place, the glass has no fixed position—but just that difference which Plücker speaks of occurs.

12599. The numbers are near enough to those with Object No. 6 (12566) to agree with the fact that both are the same glass, but the interference of some other effect, probably that of the state or polarity, is evident.

12600. A cause for many former discrepancies begins to be evident.

12601. Assuming that a state is induced, to be proved hereafter (12625, 36), then certain considerations present themselves. The Glass is not so much below water at *near* as it is at *great* distances, and yet when near ought to be more affected by the force than when distant. Therefore the water ought to be *more* altered by the magnetic power than the glass at near distances. For if altered in the same proportion, there should be no difference of result at different distances—or if altered in a less proportion at near distances, then water and glass ought to be farther apart at the near distances.

12602. For the effect of the magnet is to make the glass submitted to it become gradually more diamagnetic, i.e. it is more diamagnetic in water at the end of a constant subjection to the magnet than at the beginning, in the proportion of $3^\circ{\cdot}3$ and $4^\circ{\cdot}5$ (12581, 91).

12603. Then near water ought to be more diamagnetic than distant water; and *yet not tend to change places with it.*

12604. Would not this imply that water takes up and loses this state instantly, without which there would be change of place, and would not that go on to prove that this extra diamagnetic state is a stable state of the forces?

12605. It is probable that the *whole* of the diamagnetic condition depends upon the assumption of this state, and that it is only the fag end of the change which I am observing, and which resembles in its coming on and going off the fag end of the state of iron.

12606. Also, as Thomson, Plücker, etc. suppose, perhaps that these two state[s], the paramagnetic state and the diamagnetic state, are only conditions of one great state.

12607. In that case, bodies not near a magnet or subject to its power are not in the state.

12608. It is doubtless the state of bodies affecting light, but then how does a paramagnetic body affect light diamagnetically?

12609. It ought to be a natural state, i.e. one due to the assumption by the forces present of a *stable condition*, and not an *unstable* condition. But how does this resolve itself into the case of Thomson's perpetual motion of the rotating ball of diamagnetic matter.

12610. The state is probably assumed by all bodies, and hence they must play a part among the physical lines of forces, just as particles of matter do in electrolytes or static electric induction—and that would tend to shew that these physical lines exist among bodies as much as if their particles were particles of iron.

12611. But then query what is the state or relation of a vacuum, and the nature of its state.

12612. Magnecrystallic differences will depend upon the difference in readiness of the assumption of this state in different directions—including all the phenomena of Plücker, Tyndall, etc. etc.

12613. Now its relation to polarity. Polarity must be defined. That of conductors, either Paramagnetic or diamagnetic, cannot be the same as that of an original magnetic particle.

12614. It is strange that the glass should become a worse conductor or less able to carry on the magnetic forces by being subjected to them. Surely this points to the physical principle which is then in action. It seems contrary to all natural expectation that the forces should set up obstructions to their own transmission—but if true, the results will justify themselves (see corrections 12625, 33, 6, 9, 40, 1).

12615. *Film.* See Plateau's paper, Taylor's Memoires, v, p. 628, for a note on Hagen's remark on a film on water in capillary action.

12616. The box screws (   ) on the table gradually become loose by the vibrations that occur from carriages, etc.

12617. If the top of the box, where the suspension is, were to move to and fro, a very little alteration there would make much in the telescopic place. But I do not find that this occurs by the ordinary working of the torsion index. Perhaps some of the sudden changes may have occurred from this cause.

12618. No. 7 is a long narrow cylinder (12035) and projects

much above and below the poles—the change of distance is not the same therefore for the whole. Probably experiments with balls would be better.

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12619. The balance was left with the weights of No. 7 on it (12579, 95), the beam hanging by the wire, on the Evng. of the 28th—the telescopic place then being 9 or 8·95 (12595). This morning—all was right, but the place was 11·6, but I found it moving, probably from my approach up to it and treading round about it, bringing the floor down to its bearings under such circumstances; the motion was enough to give 10·5—11·6—10·64 etc. After stamping about it, the mean place was 10·75. Sometimes I have thought that lighting the gas light over the balance has had the effect of disturbing it.

12620. Cleared the poles—put in the water vessel—and left the weighted beam to swing—before adjusting Zero and telescopic place. This must always be done, after such preparations or brushing out. The distance is 0·7 from Tuesday last and requires no altering. The whole was brought to 0° at the Index and place 6 at the telescope, after which the torsion, being made 3° direct, was not touched for all the rest of the experiments with No. 7 object, which was now placed on the balance and in water at this distance of 0·7. Adjusted the telescope scale a little to make place 6, and after long observation and resting, found that to be the true place under present circumstances and state of the glass object.

12621. Then carried it close up into the angle of the magnetic poles by moving the beam, keeping the object in the water, and held it there a minute—set it free and watched its mode of coming to its place. Being out of sight at first—it gradually became 1—2—3—4—5—5·2—5·4—5·6—5·8—5·9—6·0—6·1—6·15 at the end of 12 or 14 minutes.

12622. Repeated the experiment, holding the object in the angle for 19 *minutes*. Afterwards the places gradually became 1—2—3—4—5—5·2—5·4—5·6—5·8—5·9—6—6·1—6·18, and there stood. So it has gained nothing either by the short or the long exposure to the magnetic force in the water.

12623. Repeated the expt., but whilst holding the object in the angle, reversed it 180° to obtain the supposed opposite state. Then put it on the beam *unreversed* at a distance, keeping the object in the water all the time. The place, at first being out, gradually became 12–10–8–7–6·8–6·6–6·4–6·2–6·15–6·15– – – 6·18– – –. So reversion at the poles appears to make no difference.

12624. Repeated–reversing at the poles and unreversing on the beam–as left on the beam the place was inwards, but it gradually became 3–4–5–5·2–5·4–5·6–5·8–5·9–5·95–6·0, etc., and the glass seems to come to the same place as well as before–finally the place became 6·1 and in half an hour more, 6·27.

12625. This difference is not the same as that obtained with the same glass on Tuesday (12590–4), and I do not believe that any effect of *state or polarity* by reversion *in water* is produced, but as this medium is so near the glass, I must try the effect as before in air (12636).

12626. Took off No. 7 object–wiped it with damp cloth–removed the water cell–held the object up in the magnetic angle–put the cell into its place and the object in the water on the balance and observed–being out, it went to 8–7–6–5·5–5·4–5·3–5·2–5·1–and then returned to 5·2–5·3–5·4–5·5–5·6–5·7–5·8–5·9–6–6·1–6·2–6·28–6·3–6·32–being then nearly still. Is this accident or an effect real in its nature?

12627. Repeated–the object being both in this and the last experiment always parallel to itself, i.e. not changing its position by reversion. When observed, it was going out from 11–12–and then in again to 10–8–6–5·8–5·7–5·68–then out to 5·7–5·8–5·9–6–and finally to 6·4. The effect like but not so great as before.

12628. Again–the object being put close up in the angle and tapped frequently against the copper sheath there for 6 or 7 minutes. The fork left it well at place 8–becoming 7–6–5·8–5·4–5·0–4·8–4·7–4·65–and then out to 4·7–4·8–and then very slowly outwards through 4·9– – –6·0–6·1–6·3–and finally 6·5. Here the effect is like as before and large.

12629. Again–but *reversed* the Object No. 7 180° only whilst in the air and magnetic angle, to give the supposed contrary state, continuing it there 6 minutes and tapping it against the

copper shield. When replaced on the beam, its place was 10·5–9–8–7–6·5–6–5·8–5·6–5·5–5·48–and then it slowly returned to 5·5–5·6–5·8–6–6·2–6·4–6·5–6·55– – –. The effect therefore is not changed by reversion of the object at the magnet, and if there is any state acquired, it would appear to be an *axial* and not a *polar* state.

12630. Now took off and out the object–moved away the water cell and did all as on former occasions *except that the object was not held near the magnet*. Replaced it and found it left in the fluid at place 7–from whence it went to 6·9–6·7–6·5–6·4– – –6–5·8–5·7–and then returned outwards to 5·8–6·0–6·1–6·2, and would have gone on.

12631. Took out the object–wiped it–did not bring it near the magnet–and returned it to the beam, *not having moved the water cell*–the place was 5 and it gradually became 6–6·2–6·4–6·5, etc.

12632. Took it out–wiped it–moved the water cell–restored it and the object–and now place 8·5, from where it moved slowly up to 8–7·5–7·2–7–6·9–6·8, etc. etc.

12633. Took it out–wiped it–held it near the magnet–tapped it, etc. etc.–restored it to the beam and water, *immersing it first* BETWEEN *the beam and the magnet* and not as on former occasion outside of the beam. A consequence is that in moving it to the beam, it will now tend to put the water in motion outwards where the object is and not inwards, and the object is large. The place when I first looked into the telescope was 11 or 10 and became 9–8–7–6·8–6·6–6·5, but went no lower, then, for it returned to 6·6–6·8–6·9–6·89, etc. I suspect the whole is due not to any assumed state, but to the motion given to the water in putting the object into its place on the beam.

12634. The object was taken out of the water and at once put in again on the outside of the beam, and the place then went up, even quickly, from higher numbers to 3, and even to 2, though there had been no exposure to the magnet.

12635. The object taken out again and restored, putting it into the water *inside* the beam. It went out to 12 or 13 and then returned 11–10–8–7–6·8–6·6–6·4–6·3–6·26 and afterwds. went out to 6·3, etc.; but all this is the effect of current and momentum.

12636. The object taken out—held at the magnet—and in again *between* the beam and the magnet—was sent out to place 10—and then became 9—8—7·7—7·5—7·3—7·1—6·9—6·8, etc. etc. So no magnetic effect here—all is effect of the current produced by the mode of putting on the object (12625).

12637. The place has been gradually changing during all the experiments from 6 on to somewhere about 6·5 (12642), but on the whole the object has finally come back to its place exceedingly well; and as this has been from both high and low places, it gives assurance as to the general principle and mode of action. Small objects will of course be far less influenced by currents than large one[s] and objects will be less influenced as they are farther from water. Even the apparent effect of distance may perhaps be accounted for in this way, by the mode of putting in the water.

12638. I now changed the object and the distance, to see what would happen with a very different body than glass, namely *bismuth*. For this purpose No. 9 (12035) was taken, and the distance made 1·5 inches, to allow of a low degree of torsion. When the torsion was 30°, the telescope scale was adjusted to the place 6 to begin with—all due time being allowed. Then it was taken off the beam—held close to the poles against the copper shield—replaced on the beam. It went out by mechanical agitation to 8—9—10, etc.—and then swung back, going on both sides of 6 freely, because of its small bulk and great weight and therefore little resistance, with much momentum—places 4—5—6—6·7—5·65—6·12—5·96—settling closely at 6.

12639. Again—as just now—but put in outside the beam—on looking in the telescope, it was far out above 16—then swung in to 2·4—then out to 7·65—in to 5·45—out to 6·35—in to 6·04—out to 6·2—etc. etc. etc. No signs of any state conferred on the bismuth here.

12640. Again—holding the bismuth to the magnet in the air in a reversed (180°) position; the places when observed were at the end of the vibrations—9—5·8—6·7—6·3—6·47—so the number increasing as before, but there are no signs of a magnetic state.

12641. Again—held to the magnet—direct—then when in the water the places were 11·4—5·1—7—6·2—6·45—etc. So here no signs of any state or polarity.

12642. The rising of the place (in number) shews that the object is farther out from the magnet, and though the amount be very small, yet the effect seems very constant when direct torsion is on, not only with the 30° (12638, 40) but even with the 3° (12620, 37), when some hours pass. It consists with and I believe is due to a little set gradually given to the wire—the wire losing of its torsion force, not being perfectly elastic. A glass suspension would be the true thing if one could have it long and strong enough.

12643. Whilst the bismuth was on the beam in water, I diminished the distance until it was about 0·6, and then with a torsion of 340°, the telescopic place was 7·2—7·6—7·4, and seemed steady at 7·4 or 7·5; being left some time, there was no sensible change—the place was exceedingly steady. But then a change in the torsion might occur equal to 2 or 3 inches on the telescopic scale when torsion near Zero and yet not shew itself, for there a degree of torsion at the Index would cause a change of from 2 to 2·5 inches on the scale; whereas now a degree of torsion at the index was [? does] not sensibly alter the place in the telescope, because the forces concerned, both of the Magnet and the torsion, are intense.

2 OCTR. 1852. Temperature 63°·5 F.

12644. Prepared for some experiments on the results at different distances with No. 6 flint glass—a piece of heavy glass No. 44 (12035)—and No. 9 bismuth. Brushed out the balance box. Made the distance 0·7. Placed the weights of No. 6 on the beam (12556, 75). Adjusted the Index at Zero—and the telescope place at 6. I took those experiments first that required the least torsion, that there might be the least error due to gradual set of the wire in the results, and the less correction to make.

12645. No. 6 flint glass cylinder (12035) in *Water*. Torsion 2°·5—then 1°·5—place varies very slowly—5·8—5·85— — —5·8—made the torsion 1°·4—place 5·86— — —5·7. If one took these data, the difference of places 6 and 5·7 is 0·3, which is equal to 0°·1 of a degree, and that would give 1°·3 as the torsion, for there is the 0°·1 too much on the wire. I made the torsion 1°·3, but the object scarcely moved—still it increased in place to 5·75—made the torsion 1°—and now it moves only very slowly—place

5·9— — — —5·95— — — —6— — — —6·02— — — —6·07— — — — 6·1— — — —6·14, exceedingly slow. The torsion is I think 1°·2. I do not think that by waiting a long while it would get more out from 6, for the 1° on, than it was in on the other side for the 1°·3 of torsion.

12646. These slow motions in a body so near water and having such feeble forces urging it on either side cause great risk of error, either *too great* or *too small*, as one has to rise or fall in place. One would never think of waiting long enough for the quiescence of the water and the settling of the body, without great experience.

12647. No. 6 at the same distance of 0·7 in *Air*; torsion force 18°·9. These numbers give the place of No. 6 as 6·78 below water.

12648. If water had been 1·5, which appeared to be too high, giving places of 5·8—5·85—5·8 etc., that would only have made No. 6 8·6 below water. If it had been 2° in water, the number would have been 11·8. The torsion force could hardly require any correction, and I have not found that any motion of the balance could make an error any thing like 0°·7 of a degree, or even a seventh part of that.

12649. Heavy glass No. 44 (12035). Its weight was 292 grains. It was a long parallellopipedon 1·14 of inch long and nearly 0·52 and 0·34 of an inch in breadth and thickness. It was suspended by a frame of copper wire, which being magnetic, will require that a correction for the torsion should be made. In the present case the distance was made 1·2 inches in *water*. The torsion was 10°—too much—8°—place 4—4·2—4·4—4·6—4·8—5·0—5·2—5·4—5·5— — —5·98— — — —by waiting, on to 6·15. I think the torsion is 8° or a little more, perhaps 8°·1. The motion is very slow and the beam very heavy—there are 5 lead counterpoises on it—this would tend to give a quick set, but there is very little torsion on the wire. Now tried the piece in *Air*—it wanted 7 lead counterpoises—the torsion was 28°·5. I found the pastboards were not closed up at the magnet but I do not think there was any influencing current admitted thereby. These numbers would give 39·7 as the place of the heavy glass below water (12651, 12658, 9).

12650. Made the distance 1·5 inches and employed No. 9 bismuth (12035) in *water*—required only two rings of lead. With 31°·5 of torsion the place was—6—6·15—6·1—6·05—6·12. So vibration free

here, and torsion perhaps not quite enough, 31°·6 probably. Made the torsion 32°, and then the place became–6–5·92–6·3–and then to and fro, settling at 6–and after a long while at 6·1. So the torsion is 32° or close upon it, but requires a correction for the attraction of the platinum suspending wire; whatever that may be will have to be added to this torsion (126). Then the bismuth was tried in *Air* at this distance of 1·5. The torsion was in succession 33°·8–had to diminish it–33°–32°·9, and this appeared to be right (12651).

12651. Proceeded to examine the wire as to set. Put on the same weights as before (12644) and made torsion index 0°. The beam
6·47 7·75 6·62
swings freely and its successive places are 0·8–12·15–2·35–
7·25 6·8 7·25 6·9
10·9–3·6–10–4·5–9·3–so 7·1 is about the place in the telescope and the set received= 1·1 on its scale, or about 0°·42 of torsion; and this amount has to be *subtracted* from the torsion observed for the bismuth, and perhaps also for the heavy glass (12650, 12649).

12652. Proceeded to No. 6, at small distance of 0·3, leaving the torsion scale as it is, so that the torsion correction will be required. In water, the torsion force being 13°, the place was 6·16–6·15–torsion force made 14°–and that being too much, made it 13°·5–the place now was 5·97– –5·99– – –. So that 13°·5 minus 0·42 (12651) gives 13°·08 as the experimental torsion in Water. Then in *Air*, for this distance the torsion was 142°·4, which corrected by 0°·42 gives 141°·98 or 142° as the experimental torsion in *Air*. These numbers place No. 6 as 10·14 below water. At the greater distance it appeared to be only 6·78 below water (12647).

12653. Now the *heavy glass* (12649) at the distance of 0·6–in *Water*. With 36°·5 torsion the place was 6·12–with 37° torsion, the places successively were 6·1–6·05–6·0– – –6·0. So the result is torsion 37°, to be diminished by the 0°·42 above (12651) and increased by the attraction of the copper holding wire (12658, 9). In air the torsion was 183°·2, requiring the same corrections.

12654. The *bismuth* No. 9 at the distance of 1·0 inch–in water–the torsion 102°·2. In *Air*–torsion 105°·9 or 106°.

12655. The bismuth No. 9 at the distance of 0·6 of an inch—in Air first, torsion 405°. Then in water 384°·3.

12656. As the two last torsions were high, I loaded the beam with the original or first weights (12644), placed the index at 0°, and

10·3 10·75 10·45 10·75

after a time found the places to be 7·4—13·2—8·3—12·6—8·9—so that the place is near upon 10·6, and a loss of torsion = 4·6 on the telescope scale or 1°·4 (2° see below) of torsion has to be made for the last results—and for the results with bismuth at 1·0 distance a correction between this of 1°·4 (see below) and the former of 0°·42 (12651) will be required.

12657. To check this calculation of the correction. Advanced the torsion index, so as to bring the scale place to 6, and found that it required 2° very nearly—certainly not 2°·1 but 2°, to compensate for the place.

12658. Considering the place 2° on the scale ring as the zero therefore for the time, I proceeded to take the heavy glass out of its copper suspension and submit the latter exactly in the same position to the magnet at the distance of 0·6. It was attracted and kept up against the inner stop with reversed torsion of 3°·5 (counting from the direct 2°)—of 4°—but with 4°·5 went off freely—with 3°·5 only, went up again—then with 4° went off—with 3°·5 goes up again, but rebounds by vibration far off—in returning, it did not go quite in but only to place 6·5 and then out, so that 3°·5 appears to be quite enough, and there seems to be an alteration in the wire since I began. I left it whilst I took tea, with only 3° of torsion on, and found it afterwds. resting quietly against the stop. Made the torsion carefully 3°·5—it did *not* go off. Made it carefully 4°—still it did *not* go off—with 4°·5 reverse torsion it did not go off. With 5° it went freely off. In these results I think there was no *sticking* nor *concussion* at the stop. So assume it as 4°·5.

12659. But now, thinking the set of 2° in the wire might be only temporary, I put the beam in equilibrium without the weight (   ), i.e. just without the copper suspension under trial, to see whether it was out 2° direct or not, and found its places not 6 but 0·4—5·6—1·6, etc. So put back the index to 1° direct and then found the places to be 7·86—4·75, etc.; so now the place is close

upon 6 and the above torsion of $4^\circ\cdot5$ must be diminished and reduced to $3^\circ\cdot5$—that being the difference when the copper suspender is off or on the beam. It is attracted with a force of $3^\circ\cdot5$ torsion at the distance of 0·6 (12649, 53).

12660. Now to consider these results as to the effect of distance. Those with No. 6 or flint glass cylinder, when with all the corrections that appear needful, give its place as

10·14 below water at distance of 0·3 (12652)
6·78 „ „ 0·7 (12645, 7)

This is the reverse of what took place before (12566, 96) with this cylinder and this glass, and seems to shew that the observations with very little torsion on the wire, in fluids so near as water and with so little force, are doubtful. Results at distances of 0·2—0·3—0·4 are the safest (12566, 96)—those at 0·7 are very doubtful (12596, 660). I do not see here any indication of distance effect. When using the Electro magnet, I had torsion up at 300° and yet nearly the same result (12231, 70). No. 7 also gave nearly the same result of 10·12—11·2 (12217).

12661. Then as to the *heavy glass* results. Those at the distance of 1·2 inches (12649) require the following correction: 0·42 to be subtracted on account of torsion (12651) and 3·5 to be added on account of attraction of the copper suspension (12659)—or 3·08 to be added as the sum of correction[s]. This makes

$$\left.\begin{array}{ll}\text{Air} & 28{\cdot}5+3{\cdot}08=31^\circ{\cdot}58 \\ \text{Water} & 8{\cdot}1+3{\cdot}08=11^\circ{\cdot}08\ [\textit{sic}]\end{array}\right\}\ \text{place below water } 54{\cdot}05$$

The results obtained at distance of 0·6 (12653) with the same correction will be

$$\left.\begin{array}{ll}\text{Air} & 183{\cdot}2+3{\cdot}08=186{\cdot}28 \\ \text{Water} & 37\ \ +3{\cdot}08=\ \ 40{\cdot}08\end{array}\right\}\ \text{place below water } 27{\cdot}41$$

The results of which are that

at distance 0·6 it is 27·41 below water
1·2 it is 54·05 below water.

This difference is enormous and looks like the first results with glass (12566, 96) but not like the last (12660). I must repeat the experiments with this heavy glass, for the lowest torsion employed is 8°.

12662. Now as to the *bismuth*. For the results at distance 0·6

(12655), they require a correction of 2° to be subtracted (12657), which makes them stand thus:

$$\left.\begin{array}{lll} \text{Air} & 405^\circ & -2^\circ = 403^\circ \\ \text{Water} & 384^\circ{\cdot}3 & -2^\circ = 382^\circ{\cdot}3 \end{array}\right\} \text{giving a place } 1846{\cdot}8 \text{ below water.}$$

Those at the distance of 1·0 of inch (12654) require a correction but less, and I take it at 1° (12656, 7), which makes the results stand thus:

$$\left.\begin{array}{lll} \text{in Air} & 106^\circ & -1^\circ = 105^\circ \\ \text{in Water} & 102^\circ{\cdot}2 & -1^\circ = 101^\circ{\cdot}2 \end{array}\right\} \text{giving a place of } 2663{\cdot}1 \text{ below water.}$$

Those at the distance of 1·5 of an inch (12650) require a correction of 0°·42 (12651), which brings them thus:

$$\begin{array}{lll} \text{in Air} & 32^\circ{\cdot}9 - 0{\cdot}42 = 32{\cdot}48 & \\ & 33^\circ \;\; - 0{\cdot}42 = 32{\cdot}58 & \\ \text{in Water} & 32^\circ \;\; - 0{\cdot}42 = 31{\cdot}58 & \end{array} \quad \left.\begin{array}{l} 3509 \\ 3158 \end{array}\right\} \text{below water.}$$

So the result stands thus:

$$\left.\begin{array}{llll} \text{at distance of } 0{\cdot}6 & \text{bismuth is} & & 1846{\cdot}8 \\ 1{\cdot}0 & \text{,,} & \text{,,} & 2663{\cdot}1 \\ 1{\cdot}5 & \text{,,} & \text{,,} & 3333{\cdot}5 \end{array}\right\} \text{below water.}$$

12663. So here also a great change in the same direction as with heavy glass, with change of distance. The better diamagnetic is more below water as the distance is greater. But a correction is wanted for all these numbers as to Bismuth in the attractive force of the Platina wire. Must ascertain that for the three distances (12681); still it will not alter the direction of the progression.

12664. In order to see if mere torsion of the wire deflects or changes the apparent place in the telescope, I hung on a lead weight equal to No. 6 by a long fine copper wire to the beam, and stopped it at the poles by a block near the true place. It was not important where, so that it was unchanged for the time. With 5° direct torsion the telescope place was 3·7—with 90° place 3·7—with 180° place was 3·6—with 270° place 3·7—with 360° place 3·65—with 180° place 3·65—with 90° place 3·65. With 10° place 3·6 or 3·65. So there is no other effect of displacement of the beam (than a little excentricity by torsion of the wire) and nothing that can account for the differences at different distances.

12665. The wire seems very well attached now but liable by softness to a set. Would a reverse torsion of equal amount each

time undo and correct it? Left the torsion index at 1° direct, which is the present Zero, or was for the light beam just before the last 360° direct torsion was on; it is on the fork and we shall see if it goes back, i.e. if it shall be found at Zero and place 6.

12666. The correction for suspensions troublesome—must use silk or bristles or glass. Theoretically, the part *in the Water* should be like the body, and the part out of the water like air; both should be as small as possible, perhaps fine silk round the body would do, and Glass loops or bristles in the air. The object No. 1 is glass not far from air, its place being 77·7 above water.

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12667. The beam left on the fork since Saturday the 2nd (12665). Now made it light, and found that now I had to put the index back from 1° direct to 0°. But after a time, the weight of the beam now hanging on the wire, the place changed from 6 to 7·3, and upon loading the beam with the weight of No. 7 (   ), it rose to 8·1, and I had to make Zero 1° direct. It must be left with its own weight and perhaps a little more on the wire.

12668. Have made some bristle suspenders, and have two fine kinds of sewing silk to go as bands round the body. A bristle suspender with a coil of the pink silk below, as an object, being put on the beam (otherwise light), displaced its place from 8·3 to 4·9, or by 3·4 on the telescope scale, which is 1°·5 nearly, being so much magnetic.

12669. Removing the silk and leaving the suspender (bristle)—it did not appear to be magnetic, but rather diamagnetic. Put 12 inches of the yellow silk in a small coil on the same suspender, and now the telescope place was 6, so it is less magnetic than the red or pink silk. When this yellow silk was off, the place of the bristle alone was 6·7, so it is somewhat magnetic, and it appears that as to the yellow silk, 12 inches of it in a small coil down at the magnet, with the distance of 0·6, changed the place only from this 6 to 6·7. When both bristle and silk off, then the place was 7·7. The bristle therefore, though raised above the poles, proves to be Magnetic, for beam place is . . . . 7·7

Bristle on it makes it . . . . 6·7

Bristle and the yellow silk . . 6

12670. Have put a bristle round the heavy glass (12649, 61), which with another for its suspender, would seem like to do. The two bristles without the glass were hung in form on the beam with the weights of No. 7 still on it, the Zero being 1°D and the telescope place 6 at this distance of 0·6. After dinner, I found the place to be 5·75—shewing a little attraction in air, but very small. Either the torsion varys with little or much weight on the beam (   ), which is not likely to this extent; or else, as is likely, bristles differ, for these are very little magnetic, and yet one is close down on a level with the magnetic poles.

12671. When put into water—the bristle round the glass gave way at the loop at the weaker or outer end of the bristle. So dismissed this bristle, and put one tie of the yellow silk round the heavy glass with a small loop above—and used a fine copper wire loop (12659) to hang it by. The beam has had the weights of No. 7 on it—and the Zero is 1°D (or direct) and the place in the telescope 6.

12672. Now took the *heavy glass* at different distances, beginning with the greatest distances and the glass in water, that there might be the least need of correction in these more delicate observations with small numbers.

12673. Distance 1·2 inches—heavy glass in *water*—with torsion 9° the place was 6·75— — —6·45— —. Made torsion 9°·5—then place was 6·3— — —6·23— —. Made torsion 10°—then place was 6·21—6·14— —6·11— —. Torsion made 10°·5—then the place became 6·06— —5°·95. So the torsion is 10°, or a little more, as 10°·1 or 10°·2, *certainly* not less than **10°**. In *air* at this distance of 1·2, the torsion was **25°·5**.

12674. Distance 1·0 inch. *In Air*—the torsion **40°·6**. In *water*—the torsion of 14° gave places 5·3— —5·42— —5·51—torsion being too much, made it 13°·5—then place opnd. out to 5·8—5·9—6·15—6·2, so this too little torsion. Made it **13°·8** and this appeared to be the truth.

12675. Distance 0·8 of an inch. In *Water*—torsion 22°·5—place successively 6·8—6·7—6·6—6·5—6·45—made torsion 23°—then 23°·5 and finally **23°·7**, before place 6 gained. In *Air* the required torsion was **72°·5**.

12676. Distance 0·6 of an inch. In *Air* the torsion requisite was

151°. In water—it was 48°— —45°— —43°— —43°·1, and this seemed right.

12677. Distance 0·4 of an inch. In *water*—the torsion in succession 103°— —105°·5— —106°·5— —107°·5— —109°— —**109°·5**— which was final. In air the torsion was **392°**.

12678. Distance 0·3 of an inch. In water first, to avoid the set of the high torsion which air would require—so in water, the torsion was **186°·5**. Then in *Air*—it was **678°**. In all these cases, the real torsion is given, i.e. the allowance is made for the Zero at 1°D.

12679. Then took off the heavy glass and replaced it by weight on the beam, to see how far the torsion has given by set through the influence of these last high twistings (12678), and found it was 2° out, the Zero being at this moment at 3°D instead of at 1°D. This correction will be required for the 0·3 results—and perhaps 1° for those at 0·4 distance.

12680. Just to see what reversing the twist would do, whilst the weight was on the beam, I gave by the Index a reverse torsion of 400° or 450° for a moment only—then brought the index to Zero—took off the weight—made the beam as light as possible—and found that when the index was at 1°D, being the first or starting zero, the place was 7·17. So that it is very nearly in its first state (12667). Still, the place keeps changing a little, as if the wire took *time* to settle amongst its particles.

12681. Broke up the bismuth No. 9 and took out the platina wire, to hang it on the beam and get its attraction at the different distances of 0·6, 1·0 and 1·5 inches (12663), for a correction to the former results, using the inner stop at the telescope place of 6 and the beam light as might be, to be level. At the distance of 0·6, the platina was attracted with a torsion force equal to 5°—but 6° sent it away and kept it away—assume 5°·5 as the attraction. With the distance of 1·0 inch—the attraction equalled 2°·6 of torsion. With the distance of 1·5 inches, the attraction was 2°·3 of torsion. When the platina and its counterpoise off, the telescope place was as at first.

12682. Now as to the results with heavy glass. Those at distances of 1·2—1·0—0·8—0·6 require I think no correction, for the torsion had never been higher than 72°·5, even for the last water observation

of 43°·1, and any set there would not be of such amount as to influence the order of the results, whatever that may be. For the next water observation, I allow 0°·6 correction, for the next air and water observations 1°, and for the final air observation 2°, according to the result at (12679). This gives the following result:

$$\text{distance } 1{\cdot}2\ (12673)\left\{\begin{array}{ll}\text{Air} & 25^\circ{\cdot}5\\ \text{Water} & 10^\circ{\cdot}1 \text{ or } 10^\circ\end{array}\right\}\text{ place } 65{\cdot}58 \text{ below water (or } 64{\cdot}5)$$

$$\text{distance } 1{\cdot}0\ (12674)\left\{\begin{array}{ll}\text{Air} & 40^\circ{\cdot}6\\ \text{Water} & 13^\circ{\cdot}8\end{array}\right\}\text{ place } 51{\cdot}5 \text{ below water}$$

$$\text{distance } 0{\cdot}8\ (12675)\left\{\begin{array}{ll}\text{Air} & 72^\circ{\cdot}5\\ \text{Water} & 23^\circ{\cdot}7\end{array}\right\}\text{ place } 48{\cdot}56$$

$$\text{distance } 0{\cdot}6\ (12676)\left\{\begin{array}{ll}\text{Air} & 151^\circ\\ \text{Water} & 43^\circ{\cdot}1\end{array}\right\}\text{ place } 40$$

$$\text{distance } 0{\cdot}4\ (12677)\left\{\begin{array}{ll}\text{Air} & 392^\circ\ -1^\circ\ = 391^\circ\\ \text{Water} & 109^\circ{\cdot}5 - 0^\circ{\cdot}6 = 108^\circ{\cdot}9\end{array}\right\}\text{ place } 38{\cdot}6$$

$$\text{distance } 0{\cdot}3\ (12678)\left\{\begin{array}{ll}\text{Air} & 678^\circ\ -2^\circ\ = 676^\circ\\ \text{Water} & 186^\circ{\cdot}5 - 1^\circ\quad 185^\circ{\cdot}5\end{array}\right\}\text{ place } 37{\cdot}8 \text{ below water}$$

There can be no mistake in this series; the numbers are in very good progression, and they prove the same fact as the former numbers and as that proved by bismuth and flint glass. The numbers or place before for this heavy glass was, at 1·2 distance, 54·05, and at 0·6 distance, 27·41 (12661). I put most trust in the latter results—i.e. in those of to-day. Still, there may be some variable influence not as yet recognised, which may presently account for this difference, for

12683. The place of heavy glass is at one time 27·41 below water and at another 40 below water—for the same distance of 0·6 (12661, 82).

12684. Then corrected the *Bismuth* results of (12662) by the amounts found to-day for the platina wire (12681). The results stand as

$$\text{distance } 1{\cdot}5\left\{\begin{array}{ll}\text{Air} & 32^\circ{\cdot}48 + 2^\circ{\cdot}3 = 34^\circ{\cdot}78 \quad \text{place } 3764{\cdot}4 \text{ below water}\\ & 32^\circ{\cdot}58 + 2^\circ{\cdot}3 = 34^\circ{\cdot}88\\ \text{Water} & 31^\circ{\cdot}58 + 2^\circ{\cdot}3 = 33^\circ{\cdot}88 \quad \text{place } 3488 \text{ below water}\end{array}\right.$$

$$\text{distance } 1{\cdot}0\left\{\begin{array}{ll}\text{Air} & 105^\circ\ + 2^\circ{\cdot}6 = 107^\circ{\cdot}6\\ \text{Water} & 101^\circ{\cdot}2 + 2^\circ{\cdot}6 = 103^\circ{\cdot}8\end{array}\right\}\text{ place } 2731{\cdot}6 \text{ below water}$$

$$\text{distance } 0{\cdot}6\left\{\begin{array}{ll}\text{Air} & 403^\circ\ + 5^\circ\ = 408^\circ\\ \text{Water} & 382^\circ{\cdot}3 + 5^\circ\ = 387^\circ{\cdot}3\end{array}\right\}\text{ place } 1871 \text{ below water}$$

So these results with bismuth give the same conclusion as those with heavy glass (12682).

12684[1]. Found the beam right as I left it, i.e. quite right and hanging quite still—the Zero being 3° D and the telescope place 9·8 or 10. Even taking off the loose holland cover had caused a little vibration—and I found about half an hour after that the telescope place had changed, being then 7 nearly—it would probably have gone back to 6. There is a gas light from a fish tail burner about 12 or 13 inches above the top of the box and index etc. and 3 inches on one side or south of it—this is lighted just as I begin to prepare. Is it the cause of this change of place at the beginning of the day's operations? After a time the kind of change ceases (12700).

12685. Weighed the oxygen tubes, etc.; their value is in the list at (12035).

12686. For tentative and preparatory results, I made the distance 1·0 and placed No. 13 (12035) (being a cylinder of *oxygen* weighing 107 grains and 2 inches long, suspended by a glass fibre the same as the body of glass) on the beam to see what room there was between it and the magnet; there was abundance, and the object being as a whole diamagnetic, keeps out with a power of 8° or 10° direct torsion. As this force is small I made the distance 0·7—still plenty of room. The water cell was in place and the object in it and attracted, being held against the *inner* stop with a power of about 85° Reverse torsion. When it is at the outer edge of the same stop, which give[s] distance of about 1·0, then it goes in when the reverse torsion is reduced to about 38°. When at the distance of 1·0, with inner stop in use, I had found it 36° reverse torsion. So these two come very well together, and give encouragement as to the working with so large a body in water.

12687. The notch in the stops is too wide, and they allow too much swing, so that time is lost and the fluid agitated. By having both stops up at once, the inner edge of the inner stop and the outer edge of the outer stop can be adjusted at any distance, so as to give an extent of vibration equal to a degree or so, i.e. to 2 or 3 inches in the telescope scale. I found this very valuable in the attraction results.

12688. Again tried the *water* results at this distance of 0·7, and with the stops as described, 88° reverse torsion takes the object

[1] 12684 is repeated in the manuscript.

from the inner stop. The diamagnetic force in *Air* at this distance was about 17°·5 direct torsion.

12689. So this distance will do, but having room and wishing for larger forces, I made the *distance* 0·6, and shall keep that for this day's work; at that distance the direct torsion with No. 13 in Air was about 25°.

12690. Took off No. 13 and placed a lead counterpoise on the object end of the beam, which compensated its loss of weight in air, so that the wire sustained as much as before, minus one of the two rings from the counterpoise end, and then adjusted the beam parallel to the sides of the box and the Zero at 0. Then made the telescope place 6. So when the beam was adjusted, the lead on it was the same as when the Object No. 13 was on it, i.e. one ring and one loop; and when No. 13 was in water, two rings and two loop[s] were again required to balance the beam (12692). In air the beam was of course heavier by the whole weight of the Object = 107 grains. The temperature is to-day at 60° F. In water the pull on the wire was the same as in air (12692).

12691. So the distance is constantly 0·6. The temperature 60° F and the Object No. 13 (12035) or *oxygen in glass*. In Air, the steps were as follows—repulsion—so direct torsion 21°·5, giving places 6·58—5·3, or a mean of 5·94; made torsion 21°·4—then places 6·4—5·52 or mean of 5·96; made torsion 21°·2—then places 5·64—6·22 or mean 5·93. Torsion 21°—places 5·72—6·24 or mean 5·98—and concluded upon torsion 21° as the real expression.

12692. Then No. 13 in *Water*—with the inner stop up—adjusted to telescope place 6. Two loops were required on the object end of the beam to compensate the immersion—the counterpoise end remaining with its two rings of lead. Now there was repulsion and the follng. successive forces employed: 80° not enough to separate the object; 100° separates when place is 6·5—90° does not separate the object from the stop, if all quiet and no percussion or torsion—or 94°. 103°·5 gained and then the object very slowly and well went out. Repeated the trials and obtnd. 101° as the result. So 102°·2 reverse torsion for the *inner stop*. Now used the *outer stop*. Adjusted to place 6—diminishing the reverse torsion from too large a quantity until that gained which the attraction

at that distance could just overcome—was in succession 120°—116°—114°, and that seemed very nearly right. By a second series of observations again had 114°. So by the outer stop the expression is 114°—by the inner stop it is 102°·2—and the mean or 108°·1 is to be taken as the attractive force at this distance.

12693. *No.* 14 (12035). Another *oxygen tube* like the last, weighing 106 grains or only 1 grain less than it. This in *Water* by the *outer* stop gave twice over 110° reverse torsion. By the *inner* stop it gave 106° reverse torsion, and the mean is 108°, which is very nearly indeed the same as for No. 13. In air No. 14 the direct torsion was 18°·8—19°—**19°·8** D, which again is not far from the former 21° (12691).

12694. No. 15. A similar glass tube (12035) weighing 108 gr. but containing within *an air vacuum*. In *air*, this gave direct torsion successively of 42°—41°—40°—39°·8 D, which appeared to be correct. In *Water*, by the inner stop, the reverse torsion was 95° R, and by the outer stop 109° R, giving the mean of 102° R.

12695. No. 10, a similar glass tube (12035) weighing 158 grains, containing *distilled water*—and also a small air bubble. In water this was attracted, shewing that the glass is magnetic as to water—with the *outer* stop up, the reverse torsion required was 18°·5 R, and with the inner stop up it was 19° R, the mean being **18°·75 R.** In *Air*, No. 10 was repelled with a direct torsion force rising up thus—134°—135°—136°·5 D.

12696. After these experiments, put one lead weight on each end of the beam as at the beginning (12690), and with the Index at 0° left it to settle—then observed the places, which in succession were 8·82—$\overset{6\cdot41}{-\,-}$—4·0—$\overset{6\cdot15}{-\,-}$—8·3—$\overset{6\cdot47}{-\,-}$—4·65—$\overset{6\cdot3}{-\,-}$—7·95, etc.; which is so little removed from place 6 that there is no correction needful.

12697. After putting the iron at the poles and putting out the gas light, left the beam suspended with these weights on until Saturday.

Considering the results with the compound systems Nos. 10, 13, 14, 15, they are generally as follows:

No. 10 *Water* and glass (12695)	in Air	136°·5	repulsion	as a whole, 12·07 above water
	„ Water	18·75	attraction	
No. 13 Oxygen and glass (12692)	in Air	21°	repulsion	as a whole, 83·73 above water
	„ Water	108°·1	attraction	

No. 14 Oxygen and glass (12693)	in Air 19°·8 repulsion / ,, Water 108° attraction	as a whole, 84·5 above water
No. 15 Vacuum and glass (12694)	in Air 39°·8 repulsion / ,, Water 102° attraction	as a whole, 71·93 above water

12698. These results shew very clearly that water is diamagnetic in relation to a vacuum, and also that oxygen is magnetic in relation to the same zero, i.e. a vacuum, but they do not shew the proportion, because the constant but unknown place of the glass and its power therefore is present in all the cases. The glass is attracted in water, and therefore above water, and this will tend to throw up or down the places of oxygen and a vacuum. The results in water may be corrected for the glass by No. 10 results, thus:

108°·1 −18°·75 gives 89°·35 as the attraction of Oxygen No. 13 in water
108° −18°·75 ,, 89°·25 ,, ,, Oxygen No. 14 in water
102° −18°·75 ,, 83°·25 ,, ,, Vacuum No. 15 in water
of course
18°·75 − 18°·75 ,, 0° ,, ,, Water No. 10 in water

but I want as yet the same correction for the results in *Air*.

12699. Air and water must both be inclosed in the same vessel to give the relation of the vessel to air and to water, i.e. to get its place in the *Air* 100 *water* scale—but after that there is no need to weigh the vessel and its contents both in air and water, and labour is only multiplied thereby, for the effect of the vessel is of no consequence provided it be constant and not too large. The *differences* give the relative places of the substances, and then making that between air and water 100° and the rest in proportion, the point is gained. Then may weigh either in air or in water, but there is no need of both. Heavy vessels are only wanted to sink into baths for temperature experiment.

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12700. Found the beam this morning as I left it (12696)—its place in the telescope 6. Then walked round it—this made it vibrate a little, and its place rose to 6·15 (i.e.—6·3— $\overset{6\cdot15}{—}$6— —). Lighted the gas burner (12684), and the place immediately began to creep up gradually to 6·4— —6·5— —6·6— —6·8— — — — —7·5, and after an hour it was standing steady at 7·3. Brushed out the

box, put on the weights which had been removed for that purpose, and finally made Zero 0°, and the telescope place 6.

12701. Returned to Object No. 13 (12035) or *oxygen tube* (12692) employed on the 7th instant. In air, its place was 18°·2 (before it was 21° (12691) but the distances, etc. have been affected since then; strictly the observations made at the same time only can be compared together).

12702. The tube was opened by the smallest aperture at the lower end, put underneath an air pump jar, exhausted—filled with air—exhausted, and filled four times in succession, so as to remove the oxygen atmosphere and replace it by one of *Air*. Then on the beam it gave a torsion direct of 31°·6.

12703. After this, it was put into distilled water, and being again placed under the air pump jar, was filled with the water—the very smallest bubble of air only being left in it, and now on the beam the torsion force was 129°·5 D. So here three

$$\begin{array}{l}\text{equal}\\ \text{vols.}\\ \text{of}\end{array}\left\{\begin{array}{lr}\text{Oxygen give} & 18^\circ{\cdot}2\,\mathrm{D}\\ \text{Air}\quad\ ,, & 31^\circ{\cdot}6\,\mathrm{D}\\ \text{Water}\ \ ,, & 129^\circ{\cdot}5\,\mathrm{D}\end{array}\right\}\begin{array}{c}\text{or water}\\ \text{made }0^\circ\end{array}\left\{\begin{array}{c}111^\circ{\cdot}3\,\mathrm{R}\\ 97^\circ{\cdot}9\,\mathrm{R}\\ 0^\circ{\cdot}0\end{array}\right\}\begin{array}{c}\text{which made}\\ \text{centigrade}\\ \text{scale is}\end{array}\left\{\begin{array}{c}113^\circ{\cdot}7\,\mathrm{R}\\ 100^\circ\ \ \mathrm{R}\\ 0^\circ\end{array}\right\}\begin{array}{c}\text{vacuum}\\ 96^\circ{\cdot}575\,?\end{array}$$

If we assume that the nitrogen of the air is as a vacuum, this would give 3°·425 as due to the oxygen in it, and place the vacuum at 96°·575 R, at the present common temperature of 57° F. The former vacuum result (12694, 7) would give a different place, namely 91·6, but the vessel is not then the same.

12704. To obtain a rough result as to the effect of temperature on things surrounded by air, I put the tube filled with water into a tube of distilled water and raised the temperature to 130° F—being taken out, wiped quickly with damp paper to avoid electricity and placed on the beam, the direct torsion was clearly too much, for the place was 5–6·2, etc. Made the torsion 121°·4—and then down to 121°—even when lowered to 120° D, that was not too little, though the tube must have been cooling all the time of observation. Its place was steadier than I expected it to be, but it must be remembered that it cooled not by evaporation but only by contact of the air, and therefore far slower than the little tube in which it had been heated.

12705. Being left, with the torsion of 120° D on, the place gradually changed, becoming in succession from 6–6·1–6·12–

6·15, etc. etc. Whilst I went to dinner, I supported the beam on the fork, and after 35 minutes, returning and letting it down,
6·7 6·67 6·69
I found the place was 7·5— —5·9— —7·45— —5·94—or 6·7 very nearly. Now put on torsion to make it place 6, and it required **132°·5**. This is more than the first torsion of 129°·5 (12703) in water at common temperature by 3°. A part of this may be due to a change in the Zero place by keeping the torsion so long upon the wire. Still, there is a difference of 10° torsion nearly between the cold and the warm water, and this must be due either to a change in the diamagnetic condition of the warmed glass and water—or to a change in the paramagnetic condition of the oxygen in the air surrounding the object and warmed by it. The *cold body* is the *most diamagnetic*.

12706. Now put the tube and its water contents into ice and water perfectly clean and pure, until its temperature was reduced to 32°, and then wiped and put on the beam as quickly as might be and observed: the required torsion was now **126°**, or less than above at common temperature, as if the cold body was *less diamagnetic* instead of more so (12705), or as if there were a maximum diamagnetic state at or near common temperatures—but that is not very likely, and the results are probably complications. As the cold object became warmer, it went out to place 6·3, shewing distinctly that the warmer body wanted more torsion force than the cold one, i.e. that the *cold body* was *less diamagnetic* than the body at common temperatures.

12707. These experiments were in Air. Now if it be supposed for a moment that the air, warmed or cooled by the body, remains round it like an associated medium, then one has to consider what ought to occur. In the warm case for instance (12704) and supposing that the body itself, like Nitrogen or Carb. Acid, undergoes no change, then it ought to seem less diamagnetic (as it did), because the air immediately around it would in its warmed oxygen have lost paramagnetic force, and so the body and its medium would be nearer to each other. But on the other hand, as the investing warm air becomes less paramagnetic, it will tend to go outwards and so carry out the object with it. I suppose that the object would tend to go in through the warm air, and the warm air out by or around the object.

But the fact is, the warm air continually goes off above and will there expend a large amount of its outward force. Is the inward force or diminished (apparently) diamagnetic condition of the object the equivalent of the force so disposed of? Or is it really in a less diamagnetic state?

12708. As regards the cold case (12706), the points would be inverted in all things except that the current of air passing away would be more paramagnetic than the surrounding air, and would dispose of a certain amount of inwd. force, and then the cold body might shew the compensation by tending to go as much outwards. This does not accord with the facts, for the cold body went inwds. (12706); but I want more and more distinct results, and only reason thus to preserve under the disadvantage of a sadly failing memory, the ideas that I may want to reconsider hereafter. The facts as far as they go are I believe good.

12709. I tried to obtain some results in *warm and cold water* thus: I put the experimental cell into a double Jacket of dry flannel; I put a thin glass tube into a double jacket of flannel and that into the experimental cell, so as to stop currents as much as possible, put pure water and pure ice into the cell so as to cool it and its contents to 32° or nearly; also put No. 13 tube containing its water into a glass tube with pure water and ice to cool it, and then put it into the inner cell. There was not room at the present distance of 0, so I was obliged to dismiss the inner tube and use the simple experimental cell. The tube and its contents were attracted, being paramagnetic to water. When at place 1·4 in the telescope, it required 90° reverse torsion to separate it and send it out. When the temperature of the cell water was 45°, the telescope place 6 and the torsion 0°, the object went in. Even when the torsion reverse was 10° and place 6, still it went in, so that this *cold object, the Glass*, was *paramagnetic* in water.

12710. Then I arranged the cell in like manner with the same object and hot water, i.e. at 130° F. The object went out as if diamagnetic, and in fact required 82° direct torsion to keep it at place 6. When at temperature 103° F it required 65° direct torsion. As the temperature fell, the necessary torsion diminished and, being once at 41°, even that at one moment seemed too much—and yet being left, the telescope place gradually went out to

7— — —8— —and even 8·4, as if 41° torsion was too little—after a little while the place seemed to be 8. *But I fear much the effect of currents* in the water, cooling as it is at the large surface and so having much motion in it by change of place. When the temperature was 80° F, the torsion seemed to be 32° direct with place 6. Still I fear the currents and the effects at 41° torsion are inconsistent.

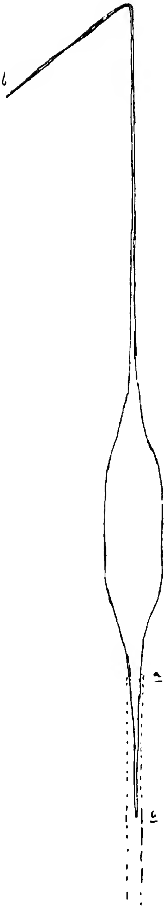
12711. Now took distilled water at common temperature, 58° F., in the cell, with the object at the same temperature, and found that it was attracted. With the inner stop up and place 6, the reverse torsion required came out as 14°, and with the outer stop up it came out as 15°. So the glass is paramagnetic in water at common temperatures with a force of 14°·5 at 58° F. Whether it becomes less so or diamagnetic by elevation of temperature is uncertain. The water experiments cannot be trusted (12710, etc.). The experiments in air (12705) favour the notion that it does.

12712. Hot water is very inconvenient in the balance box, sending up steam which obscures the reflector and the windows, and which also may condense on the magnet. I oiled the magnet in the box well to-day after the above experiments.

12713. Object No. 7, solid glass (12035), which stands at about 11 below water on the centigrade scale by former experiments, and which now in distilled water at about 60° F was distinctly diamagnetic and went out, being made ice cold in ice and water, was then put quickly and quietly into the same water at 60° just used, to see if any effect would result from its comparitive low temperature at the moment. It was still diamagnetic and, as far as I could tell, about as before. Then put on direct torsion and found that with 9° it went up to the inner stop, at place 4·6, and just rested there—was so to say quite loose. Then made the object hot in water nearly boiling and again hung it in place in the cell water at 60°, and very successfully and quietly, but the torsion effect was just as before. I saw no trace of the effect of temperature in such experiments as these and therefore still fear the influence of currents in the former results.

12714. After all this working, I find myself greatly limited by the force of currents in fluids, and do not think I can use baths of liquids, except at common temperatures, i.e. in atmospheres

at the same temperatures as the baths. But some things I may do with the balance. I. I may try several gases and fluids in a thin glass vessel at the same distance, as 0·6, including air and water as standard references. II. I may compare certain selected things at different distances and with different powers at one common temperature. III. Perhaps I may be able to compare oxygen at *common* and *low* temperatures by the use of a carefully constructed cold air bath with a small aperture above for the supporting filament.



12715. Balance box brushed out yesterday–beam has been continually on the wire with a lead loop and a lead ring on it, and the adjustments are now to Zero at 0° on the scale with telescope place at 6. The temperature to-day of the place and objects is 57° F.–distance 0·5.

12716. Have constructed a new vessel (12768), i.e. a glass bulb drawn out above and below as represented, and bent carefully at the hook so as to be round and well shaped there, that it may hang justly on the hook of the beam. The glass was thin–flint glass–and the vessel, No. I, weighed 92 grains–when filled with water it weighed 189 gr. At first, being open at the ends, it was full of air, and being placed on the beam, its diamagnetic force *in Air* was ascertained. Then a gas, as Nitrogen, was introduced thus: an air jar filled with it was furnished with a small flexible tube of vulcanized caoutchouc, the lower end of the vessel was inserted into the end of this tube *a*, which by its elasticity made a tight joint round it–then the gas was by pressure driven through it–and whilst a little pressure was still kept up, a spirit lamp was applied at the point *b*, which instantly sealed it–and then the vessel being withdrawn from the tube, the lower aperture *c* was sealed in like manner. Each end was then examined by a lens to make sure that they were well sealed, after which the surface of the glass was carefully [wiped] over with a damp camels hair brush, kept for that purpose between damp cloths, to remove any electric charge given by the hand or holding forceps or caoutchouc tube. Then it was hung on the beam and the torsion required for this new charge at the standard distance of 0·5 ascertained. This being done–the vessel was taken down–the extreme points nipped off–the contents blown out by air driven in from a vessel–the points if necessary drawn out a little by a small spirit lamp flame–the vessel filled with another gas–sealed up and weighed as before. In this operation, the portion of glass removed each time from the extremities was not the $\frac{1}{20}$ of a grain, and could cause no sensible difference in the results, and the vessel was always well filled with gas at the given temperature and that well retained for the time. In this way the following

gases were experimented with in the following order, the circumstances remaining the same for the whole time. Because of the dominant influence of the vessel, the effect was always repulsion even with oxygen, and therefore the torsion always direct.

12717. *Air*–50°–49°·7.

12718. *Nitrogen*–58°.

12719. *Oxygen*–4°–4°·5–4°·7.

12720. *Carbonic acid gas*–57°–59°–58°·8.

12721. *Carbonic oxide*–60^{c}–58°·8.

12722. *Olefiant gas*–58°·5–57°·2. This gas was very distinctly less than the two previous gases–if an effect had been due to any loss of torsion by set of the wire, it would have been in the contrary direction, i.e. the torsion would have been apparently greater.

12723. *Nitrous oxide*–59°·5–59°·6–decidedly more than the Olefiant gas.

12724. *Nitric oxide*–43°–42°–42°·2–striking difference between this and the last or Nitrous oxide: is more paramagnetic than air.

12725. *Hydrogen*–58°·5–59°.

12726. *Sulphuretted hydrogen*–59°·1–a very good specimen of the gas.

12727. *Ammonia* gas–60°.

12728. *Nitrous acid* vapour–59°–57°·5. This atmosphere was sent through by attaching the vessel to a little tube containing condensed Nitrous acid vapour (from Nitrate of lead)–at the common temperature there was a little deposition of liquid in the bottom of the vessel but not much. At the common temperature, the vapour in the vessel could not have the full pressure of one atmosphere, but was not far off.

12729. *Water, distilled*–when full the vessel weighed 189 grains. 318°·5–318°–which minus 0·7 (12731)=317°·3.

12730. *Steam* vacuum 58°–58°·8, which minus 0·7 (12731)=58°·1. A little water was left in the vessel after this vacuum was made, and though below, must in some degree have increased the diamagnetic force of the whole. The vessel was opened afterwds. and the vacuum found to be a very good one–perfect in that respect. The result makes it necessary to see whether such a gas as nitrogen is not a little more diamagnetic than its vacuum–or whether a water vacuum and a nitrogen vacuum are the same.

12731. Now placed the same charge on the beam as at first (12715) and adjusted the index until telescope place was at 6. Then the index was at $0^\circ{\cdot}7$D, i.e. the wire had given way by set so much, and I doubt not by the high torsion needed for the water (12729), so I have made this correction for its result and the vacuum result following it.

12732. Broke the hook of the last vessel No. I (12716, 68). Took a like *new one*, No. II, made of the same glass tube, and began a fresh series of experiments—the distance 0·5—the temperature 58° F—the Zero $0^\circ{\cdot}7$D. In the results recorded below, account is taken of this quantity. No. II weighed 88·5 gr. filled with air, and 176 grains filled with water.

12733. *Air*—42°—44°—$44^\circ{\cdot}2$—$43^\circ{\cdot}5$.

12734. *Sulphurous acid gas*—53·5—55°—56°—$55^\circ{\cdot}3$.

12735. *Muriatic acid gas*—$53^\circ{\cdot}5$—56°—55°—$54^\circ{\cdot}3$.

12736. *Chlorine*—53°—54°—$54^\circ{\cdot}2$—$53^\circ{\cdot}5$.

12737. *Cyanogen*—61°—56°—55°—$55^\circ{\cdot}2$—$54^\circ{\cdot}5$.

12738. *Oxygen*—7°—$8^\circ{\cdot}6$—$8^\circ{\cdot}1$—$7^\circ{\cdot}3$.

12739. *Water distilled*—301°—$300^\circ{\cdot}2$.

12740. *Alcohol common*, S.G. 0·842 at 68° F—255°—264°—$263^\circ{\cdot}2$.

12741. *Alcohol absolute*, S.G. 0·816 (12035)—240°—245°—250°—$254^\circ{\cdot}2$.

12742. Again took the Zero place of the beam with its former load and found it as before at $0^\circ{\cdot}7$D. So no further correction required.

12743. Here of course for each series the medium and the vessel are the same, and therefore the differences are due to the difference of the *contents*. Air and water have been included in both series in order to obtain the same natural standard, and the centigrade scale in both. The next page[1] contain[s] the results entered in para-diamagnetic order—the first column of figures gives the experimental results—the second column the same with water at Zero, obtained by subtracting the numbers of a series from the number of Water in that series—the third reduced in proportion so as to make Air 100°, Water still being Zero—the fourth column gives Carbonic acid and oxide as Zero, for I believe that they are close upon what a vacuum will be as they are close upon Hydrogen

[1] See pars. 12744, 12745.

and sulphuretted hydrogen—so that column shews bodies as paramagnetic and diamagnetic, a vacuum being *Zero.*

12744.	degrees of torsion	differences with water Zero	differences with water 0°, Air 100°		FIRST SERIES VESSEL I
Oxygen	4°·7	312·6	116·8	20·2	
Nitric oxide	42°·2	275·1	102·8	5·2	
Air	49°·7	267·6	100	3·4	
Olefiant gas	57°·2	260·1	97·2	0·6	
Nitrous acid vapour	57°·5	259·8	97·1	0·5	
Nitrogen	58°·0	259·3	96·9	0·3	
Vacuum Steam	58°·1	259·2	96·86	0·26	
Carbonic Acid gas	58°·8	258·5	96·6	0·0	
Carbonic oxide gas	58°·8	258·5	96·6	0·0	
Hydrogen	59°·0	258·3	96·5	0·1	
Sulphuretted hydrogen	59°·1	258·2	96·5	0·1	
Nitrous oxide Gas	59°·6	257·7	96·3	0·3	
Ammonia	60°·0	257·3	96·1	0·5	
Water	317°·3	0·0	0·0	96·6	

12745.					
Oxygen	7°·3	292·9	114·1	17·5	SECOND SERIES
Air	43°·5	256·7	100·0	3·4	VESSEL II
				zero 0·0	
Chlorine	53°·5	246·7	96·1	0·5	
Mur. Acid Gas	54°·3	245·9	95·8	0·8	
Cyanogen	54°·5	245·7	95·7	0·9	
Suls. Acid Gas	55°·3	244·9	95·4	1·2	
Alcohol Absolute (S.G.)	254°·2	46·0	17·9	78·7	
Alcohol common (S.G.)	263°·2	37·0	14·4	82·2	
Water	300°·2	0·0	0·0	96·6	

Degrees 100 between Air and Water, but Zero at Carbonic acid gas, etc.—those above marked thus ——, paramagnetic; those below, diamagnetic. Expect that true vacuum will be as C. Acid.

12746. It is evident there is an error about oxygen of the first series, or else some peculiar effect. It ought to be about five times Air, as containing five times its oxygen, whereas it is 3° more. In the second series it is much nearer. The torsion is so near Zero that the observation requires great care and perfect absence of currents in the box. In the first series, the torsion is

only $4^\circ{\cdot}7$ and in the second $7^\circ{\cdot}3$; either is very low and the truth is probably between the two.

12747. Probably Nitrogen–Nitrous oxide–Hydrogen–Sul. hydrogen–Ammonia–Chlorine–are all as a vacuum. Olefiant gas and Nitrous acid vapour may be so too, for a little common air would carry oxygen into one and the other may have oxygen evolved by the heat necessary to send it in, and that would account for their apparent paramagnetic state. Sulphurous acid gas–Muriatic acid gas and cyanogen are all more diamagnetic than I should have expected could be due to any thing left in them.

12748. The general process is a very good one and the experiments not at all difficult to make.

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12749. Continued the Experiments of the 19th in like manner, still employing vessel No. II (12732, 68) at the same distance of 0·5. The temperature is 58° F. The beam has Zero at 0° and place 6 in the telescope.

12750. *Air*–43°.

12751. *Oxygen*–$7^\circ{\cdot}7$–$7^\circ{\cdot}5$–$7^\circ{\cdot}7$.

12752. *Ether*, S.G. 0·734 (12035)–238°–237°.

12753. *Sulphuret carbon*–297°. All the fluids required four ring counterpoises on the end of the beam–by shifting their places they balanced the vessel in every case.

12754. *Sulphuret carbon–saturated with sulphur*–314°–315°.

12755. *Water*–$289^\circ{\cdot}5$–not so heavy as the last fluid–weight of the vessel and water is 176 grains.

12756. *Ammonia solution*, S.G. 0·967–$294^\circ{\cdot}2$.

12757. *Nitric acid*–pure–no iron. S.G. 1·42–258°–colourless.

12758. *Hydrochloric acid* sol.–iron. S.G. –$316^\circ{\cdot}2$.

12759. Saturated solution *common salt*. No iron–is heavier than the last solution of hydrochloric acid–321°–$324^\circ{\cdot}5$.

12760. Sat. solution of *Nitrate potassa*. No iron–lighter than the last solution–$298^\circ{\cdot}7$–$298^\circ{\cdot}1$.

12761. Sat. solution of *Alum*, No. 1–contains a trace of iron, not much–lighter than the last solution–$299^\circ{\cdot}4$–302°.

12762. Since beginning with these high numbers (12753), I have in each case, at the end of the observation and before taking the charge or weight off the beam, carried the torsion index back to zero and beyond that by reverse torsion as far on as the direct torsion had been, and then back to Zero—from whence I started at each new observation; and this was done to compensate for any set in the direct torsion of the wire and to start as it were from the same condition each time. As to this set, the following effect was observable, which I will illustrate by the alum solution. On carrying the torsion carefully and steadily on, so as to bring the beam into the telescopic place well and at once, 299°·4 was found to be quite torsion power enough, but being left a few minutes it lost place and went back from 6 in the telescope and required then 302° to bring it up—after this it did not sensibly lose any more in a moderate time. Now I believe this is due to a little set in the wire, which set appears to go out by the reverse torsion spoken of above, for it occurs again and again. I believe it proportionate also in some degree to the amount of torsion and also to the weight on the wire. The particles of the wire seem as it were a little loose on each side of their quiescent state (i.e. the wire is not perfectly elastic as glass would be), but still have not that permanent set which would prevent their return to their first position when the torsion is off.

12763. Saturated solution *Muriate of Ammonia*. No iron—a little heavier than the last or Alum solution—314°—317°.

12764. Sat. solution *Phosphate Soda*—no iron—307°—309°.

12765. Sat. solution *Carb. Soda*—no iron of course—heavier than the phosphate—314°—plenty at first—afterwds. required 317°.

12766. Here I broke tube No. II, and so an end of the vessel and of this series.

12767. Examined the Zero when the two ring weights were on the beam and its place at 6; then the Zero was 1°·1 D, so that the wire has received so much set. This is not of much consequence in the high numbers of the solution[s] above given, but would be in the first results with air and gases now to be given with a new vessel, so must take account of it. I did not alter the place of the ring scale.

12768. Another tube, No. III, made of the same glass as the former two but not quite so large. Its weight full of air was 81 grains—its weight full of water 159 grains. So—

Vessel I held	97	grains of water or	0·384	of a cubic inch.
IX ,,	71·6	,, ,,	0·2836	,, ,,
II ,,	87·5	grains of water—	0·346	,, ,,
III ,,	78	grains of water—	0·309	,, ,,
IV ,,	73	,, ,,	0·2892	,, ,,
V ,,	119	,, ,,	0·472	,, ,,

12769. *Air* in vessel No. III—42°·4—1°·1 = **41°·3**.

12770. *Oxygen*—11°·1—1°·1 = **10°**—examined the beam with the two weights and still found Zero 1°·1 (12767), so all right so far.

12771. *Nitrogen*—by Phosphorus [illegible] and freshly made—a little nitrous gas added, about 1 vol. to 60 of the Nitrogen, produced faint red fume, shewing a little oxygen in the nitrogen—let this all stand for a few hours and then used this as the Nitrogen for the vessel—50°·8—51°—1°·1 = **49°·9**. Had reason afterwds. to fear that one termination had been a little open, so will try them again in No. III.

12772. Water—270°—271°; corrected **269°·9**.

12773. Saturated solution of another Alum, No. 2, crystallized—contains also iron a trace (12761)—270°; corrected **268°·9** (268·8)[1].

12774. Sat. sol. *Sulphate soda*—no iron—282°·5; corrected **281°·4** (281·2).

12775. Sat. sol. *Nitrate lead*—a little iron—heavy—278°·4—280°·5; corrected **279°·4** (279·1).

12776. Sat. sol. *acetate lead*—no iron—278·7—corrected; **277°·6** (277·2).

12777. Sat. sol. *Bichromate potassa*—260°—260°·5—corrected; **259°·4** (259°).

12778. Examd. the Zero—found it now 1°·5 D.

These cases arranged as before (12744) give the following results:

[1] Numbers in brackets are in pencil in the MS.

12779. THIRD SERIES VESSEL II

	degrees of torsion	differences, Water Zero	differences W. 0°, Air 100°	
Oxygen[1]	7°·7	281·8	114·3	17·7
Air	43°	246·5	100	3·4
			Zero	
Ether	237°	52·5	21·3	75·3
Nitric acid sol.	258°	21·5	8·64	87·96
Water	289°·5	0·0	0·0	96·6
Ammonia sol.	294°·2	4·7	1·9	98·5
Sulphuret carbon	297°	7·5	3·04	99·64
Nitre solution	298°·1	8·6	3·48	100·08
Alum sol. No. 1	302°	12·5	5·07	101·67
Phos. soda sol.	309°	19·5	7·91	104·51
Sulrt. Carbon and Sulphur	315°	25·5	10·34	106·94
Mur. Acid sol.	316°·2	26·7	10·82	107·42
Mur. Amm. Sol.	317°	27·5	11·15	107·75
Carb. soda sol.	317°	27·5	11·15	107·75
Chlo. sodium sol.	324°·5	35	14·2	110·8

12780. FOURTH SERIES VESSEL III

Oxygen	10°	259°·9	113·7	17·1
Air	41°·3	228·6	100	3·4
Nitrogen	49·9	220·0	96·23	0·37
			Zero	
Bichromate Potash sol.	259°	10·9	4·77	91·83
Alum. Sol. No. 2	268·8	1·1	0·48	96·12
Water	269°·9	0·0	0	96·6
Acetate lead sol.	277°·2	7·3	3·19	99·79
Nitrate lead sol.	279°·1	9·2	4·02	100·62
Sul. soda sol.	281°·2	11·3	4·94	101·54

100° between Air and Water—Zero at C. Acid of former Expts.—those underlined paramagnetic.

12781. The second series (12745) and the third series (12779) are results obtained in the same vessel No. II—and the three substances oxygen, air and water are contained in both series—the oxygen is nearly alike in both, namely 7°·3 and 7°·7—the air is nearly alike, namely 43°·5 and 43°—but the water differs so much there must be some error—300°·2 and 289°·5 or 10°·7. This is probably the reason why the oxygens appear different, and I think on the whole the water of this series third is in error. Now though

[1] A note in pencil across these "third series" results reads: "Query the water of this series and other corrections (12781)."

the vessel II is broken, yet as Sol. Alum was taken in it, and is close to the result with water in the fourth series, but differs from that in the third, I may by using that solution and water in vessel III, or any other, see which of the waters in series *two* and *three* is right (   ); then the other results will want correction accordingly.

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12782. The box was well brushed out last night. This morning the distance was 0·5. The Zero place made 0°. The telescope place 6—the scale, telescope, etc. being not quite in the same place as before but giving a nearer position. The temperature was 61°·4 F. The vessel No. III was used—so that the results below are a continuation of those in series fourth (12780), except that the real place of the vessel is nearer the magnet when telescope place is 6.

12782[1]. Air in vessel—51°.

12783. Oxygen—11°·1. Then unfortunately broke the loop of the vessel. Still, lengthened the stem and made a fresh loop for the purpose of procuring the expression for Water and No. 1 Alum solutions as above (12781). They were:

12784. Water—340°·5.

12785. Alum sol. No. 1—345°—345°·5.

After that I took in the same vessel Strong *Sulc. Acid*, containing some iron by ferro pruss. pot.—352°—348°·7. When the acid was afterwards neutralized by excess of Ammonia the iron did not appear except as

12786. Now these numbers arranged as a series as before (   ) give:

VESSEL III
SERIES FIVE

	Expt.	Water 0°	Water 0° Air 100°	Space 0°
Oxygen	11°·1	329·4	113·7	17·1
Air	51°	289·5	100·0	3·4
Water	340°·5	0·0	0·0	96·6
Sol. Alum No. 1	345°·5	5·0	1·73	98·33
Sulc. acid, little iron	348°·7	8·2	2·83	99·43

[1] 12782 is repeated in the manuscript.

12787. The oxygen and air come out very close. The alum is the wrong solution; it should have been No. 2 solution. No. 1 alum differs from water—12°·5 in series third, and above only by 5°, which would go to shew that water was wrong there. Alum No. 1 differs in the third and fifth series by 43°·5, but then all the fifth series ( ) are high because the object is a little nearer. Still, none have such a great difference as the waters, 289°·5 and 340°·5, or 51°, and that again indicates that water is probably too high there.

12788. A new vessel IV taken, like the former three and made of the same glass (12768). Its weight in air when full of air was 81 grains and its weight full of water 154 grains—hence the water is 73 grains and the capacity therefore 0·2892 of a cubic inch.

Vessel IV—distance near 0·5. Temperature 61°·4 F.

12789. *Air*—42°·2.

12790. *Oxygen*—9°—8°·5—8°·7; a second charge of oxygen gave the torsion as 9°—8°·7—8°·6 or 8°·65.

12791. *Nitrogen*—exhausted of Oxygen by N.G. (12771)—58°·4—58°·1.

12792. *Water*—305°—306°. Come now to high torsions—the torsion of 306° was retained very steadily.

12793. Sol. sat. *Yellow ferro pruss. potash*—307°·5—311°·5.

12794. Sat. Sol. *Sulphate of copper*—was magnetic. When the inner stop was up at place 5·3, being of course nearer than place 6, it sustained a reverse torsion of 335° before the object went out. This was only for a rough indication. When all the contents of the tube were neutralized and excess of ammonia added to dissolve the copper, a little precipitated per oxide of iron appeared. I rather think the pure cupreous solution is paramagnetic, since this solution was attracted so strongly in air.

12795. Began with essential oil now, but examined Zero first and found it to be at 1°D when place made 6 in the telescope—so correct the ensuing numbers by this amount. Required 3 weights as counterpoises on the further end of the beam.

12796. *Oil of lemons*—263°·2—1°= **262°·2**.

12797. Oil of lavender—a little heavier than the last—290°·4—289°—1°= **288°**.

12798. Oil of Sassafras—heavier than the last—282°—1°= **281°**.

12799. *Essential oil of Almonds*—lighter than last—272°·5−1° =**271°·5**.
12800. *Camphine*—lighter than last—not quite full in neck—267°—271°—**270°**.
12801. *Cajeput oil*—heavier than last—282°·5—285°·4—**284°·4**.
12802. Essential oil *Cloves*—heavier than last—285°—286°·6—**285°·6**.
12803. Essential oil *laurel*—lighter than last—276°·4—279°—**278°**.
12804. *Aniseed*—heavier—272°·3—274°·7—**273°·7**.
12805. *Oranges*—lighter—287°·4—284°·5—**283°·5**.
12806. *Cinnamon*—heavy—264°·5—266°—**265°**.
12807. *Chamomile*—light—265°·2—268°·6—**267°·6**.
12808. *Spike lavender*—heavier—289°—**288°**.
12809. *Jessamine*—282°·3—283°·6—**282°·6**.
12810. *Carraway*—265°·6—**264°·6**.
12811. *Cubebs*—lighter—294·8—**293°·8**.
12812. *Copaiba*—thick—300·3—**299°·3**.
12813. *Volatile Mineral Naphta* from . S.G. ·715—light compared to the oils above—264°·4—**263°·4**.
12814. Coal gas Naphtha—heavier—268°·5—269°·5—**268°·5**.
12815. Rectified oil gas liquor—same weight as the last or coal gas naphtha—257°—256°—**255°**.
12816. *Olive oil* fixed—273°·5—278°—**277°**.

Examined the Zero—it is still at 1°D.

12817. When a fluid had been employed, i.e. amongst these oil[s]—then the contents were drawn out under the air pump receiver. Next by heat and cooling a little absolute alcohol was introduced—warming and cooling this, the vessel was half or two thirds filled with the alcohol—this well shaken and then driven out and then again the vessel filled ⅔rds with fresh alcohol, well washed with it, then this driven out and the vessel being full of vapour of alcohol at the boiling point, was dipped into the fluid next to be used, and so on the condensation of the vapour the fluid entered. In this way not only was the bulb perfectly filled, but more than an inch of the stem above and all the stem below. The process was a very good one—a little absolute alcohol was of course present in each case.

12818. These results are arranged in the next page[1] as on former occasions. The nitrogen seems the most out—there may be a little excess of N. Gas in it, or some nitrous vapour (12771), but that is not enough to account for the difference. It comes out 2°·63 below zero on the centigrade scale.

12819. Has the varying temperature of oxygen and the air anything to do with these irregularities?

12820. SIXTH SERIES VESSEL IV (12788)

	Expt.	Water 0°	Water 0 Air 100°	Vacuum C.A. Gas 0°
Sat. sol. Sul. copper	paramagnetic			
Oxygen	8°·65	297·35	112·7	16·1
Oxygen	8°·7	297·3	112·7	16·1
Air	42°·2	263·8	100	3·4
Zero supposed	. . . .	. . . .	. . . .	. . . .
Nitrogen	58°·1	247·9	93·97	2·63
Rectified oil gas liquid	255°	51	19·33	76·67
Ess. oil lemons	262°·2	43·8	16·6	80
Vol. Mineral Naphtha	263°·4	42·6	16·14	80·46
Ess. oil carraway	264°·6	41·4	15·7	80·9
„ cinnamon	265°	41	15·54	81·06
Coal gas Naphtha	268°·5	37·5	14·21	81·79
Ess. oil chamomile	267°·6	38·4	14·56	82·04
Camphine	270°	36	13·64	82·96
Ess. oil Almonds	271°·5	34·5	13·07	83·53
Olive oil fixed	277°	29	11	85·60
Ess. oil laurel	278°	28	10·61	85·99
„ aniseed	273°·7	32	12·14	84·46
„ Sassafras	281°	25	9·47	87·13
„ Jessamine	282°·6	23·4	8·87	87·73
„ oranges	283°·5	22·5	8·53	88·07
„ cajeput	284°·4	21·6	8·18	88·42
„ cloves	285°·6	20·4	7·73	88·87
„ lavender	288°	18	6·82	89·78
„ spike lavender	288°	18	6·82	89·78
„ cubebs	293°·8	12·2	4·62	91·98
„ copaiba	299°·3	6·7	2·54	94·06
Water	306°	0	0	96·6
Sat. sol. yellow ferro pruss. pot.	311°·5	5·5	2·08	98·68

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12821. Worked. The box was brushed out last night and the beam left suspended as usual now, with the two weights upon it.

[1] I.e. par. 12820.

The irons also at the poles to make a good keeper. The temperature this morning is 55° F.

12822. After lighting the gas, the Zero seemed to change, and it was full two hours before I had things steady. Putting away the stops seemed to make a difference, so I went over all the glass with a damp cloth and over the beam with a damp camel hair brush. After about 2 hours, began to work as things seemed steady. Used vessel IV. Distance same as has been used constantly lately, i.e. 0·5. Temperature 57° F. Zero is at 0·6 R. So 0°·6 has to be added to the observed quantities.

12823. *Air*—$44^\circ + 0^\circ 6 =$ **44°·6**.

12824. *Oxygen*—fresh made—4°—corrected is **4°·6**. On the last occasion it was 8°·7 in this same vessel. This may be due to the better quality of this oxygen than of that then used. I warmed the tube in my hand and then observed it, but it was still at 4°.

12825. *Nitrogen*—fresh made by plenty of phosphorus—when tested by a little N. Gas, it gave an exceedingly faint orange tint; the results were 52°·3—52°·4= **53°**.

12826. *Nitrogen.* Some of the above gas had a little nitrous gas added to it, about $\frac{1}{100}$, and then left for some time, an hour or more, over water. This Nitrogen gave 52°—52°·3= **52°·9**. So very like the last.

12827. *Carbonic acid gas*—fresh and good—by N. Gas gave no signs of oxygen. 55°—54°·7= 55°·3. It is clearly more diamagnetic than Nitrogen.

12828. *Air*—again 44°—43°·7= 44°·3. So that torsion cannot have been changed much during these experiments. After this, proceeded to experiments requiring high torsion. Both in these and those just made, after completing an observation and whilst the object was suspended by the wire, the torsion index was carried back to Zero and then as far on *reverse* as it had been before direct, held for a second or two and then restored to Zero. After which the fork took up the beam and all was ready for a succeeding experiment.

12829. *Water*—318°—319°—320°·5= **321°·1**.

12830. *Ammoniacal sol. of copper*—heavy compared to water. Magnetic. When the inner and outer stops were up and adjusted so that the space between them was equal to 0·3 in the telescope

scale, and the place 6 in the middle of that interval, it was more easy to determine the torsion which corresponded to the medium place or 6, than when the stops were used separately. I found this to be 240° R by the one observation. When using the stops separately, the inner gave 195° R and the outer 278° R, which are very far apart and startled me, but the mean of the two is 236°·5 R, which is exceedingly near to 240° R. Still, the observation by the two stops is easier and shorter than separate observations by each stop.

12831. The preparation was intended to throw out iron and was made thus. A strong sol. of ammonia had crystals of sul. copper put into it which at first dissolved; but as the ammonia disappeared and the liquid became scentless, the solution became turbid and dull. Then ammoniacal gas was thrown in until all became clear and being a little warm, it was filtered, leaving iron on the filter, and the clear solution was left to cool; it deposited an abundant crop of fine deep blue crystals, and the clear mother liquor or saturated solution is that employed above. I have no doubt that copper is a magnetic body since this preparation of it is magnetic.

12832. Sat. Sol. *Alum* No. 1 (12761)—317°·3—316°·5= **317°·1.**

12833. Sat. Sol. *Alum* No. 2 (12773)—331°—329°·5= **330°·1.**

12834. *Poppy oil*—291°—297°·5=**298°·1.**

12835. *Linseed oil*—287°—289°=**289°·6.**

12836. *Cod liver oil*—281°·5—284°·5=**285°·1.**

12837. Elaine from hogs' lard--293°·5—296°= **296°·6** (12874).

12838. *Sperm oil*—295°·4=**296°.**

12839. *Almond oil*, fixed—287°—291°=**291°·6.**

12840. *Castor oil*—299°=**299°·6.**

12841. *Resin oil*—295°=**295°·6.**

At the close of the day the Vessel IV was left with alcohol in it.

12842. The beam with its two weights on it and the Zero at 0·6 R was left to settle and gave places—7·65—7·08—or 7·37 as the mean—so it has altered about 1·37, or nearly half a degree, which would bring the Zero very nearly to 0°. Left it so, putting no irons about the poles; but extinguishing the gas light, allowed it to take up such change as might come on during the night. Now 5 P.M.

12843. The above results arranged—Vessel IV—distance 0·5. Temperature 57°.

SERIES SEVEN: VESSEL IV (12788) DISTANCE 0·5

	Exp. results	Air to water = 100	C.A. Gas zero	Nitrogen zero
Amm. Sul. copper	240° rev.	86·7 rev.	106·7	105·84
Oxygen	4°·6	1·66	18·34	17·48
Air	44°·6	16·1	3·9	3·04
Air	44°·3	16·0	4·0	3·14
Nitrogen	52°·9	19·12	0·88	0·02
			Zero	—
Nitrogen	53°·0	19·16	0·84	0·02
Carb. acid gas	55°·3	20·0	0·00	0·86
Cod liver oil	285°·1	103	83	83·86
Linseed oil	289°·6	104·7	84·7	85·56
Almond oil	291°·6	105·4	85·4	85·26
Resin oil	295°·6	106·8	86·8	87·66
Sperm oil	296°	107	87	87·86
Elaine (hog's lard)	296°·6	107·2	87·2	88·06
Poppy oil	298°·1	107·7	87·7	88·56
Castor oil	299°·6	108·3	88·3	89·16
Sol. Alum No. 1	317°·1	114·6	94·6	95·46
Water	321°·1	116	96	96·86
Sol. Alum No. 2.	330°·1	119·3	99·3	100·16

12844. In reference to these results, I think the Nitrogen results are very good and yet that the Carbonic acid gas is really diamagnetic to it. In which case, both cannot be at Zero. The results as regards the relation of oxygen and air is evident by the table. With Carbonic acid gas as Zero—five times air gives more than oxygen, making oxygen more than five times as magnetic as air, which is not likely, and therefore I think C.A. Gas will prove to be diamagnetic to a vacuum. Taking nitrogen as zero, then the fourth column of figures gives five times air as less than oxygen, which would either seem to shew that the nitrogen contained some oxygen—and yet the results with both nitrogens agree (12825, 6)—or that nitrogen is a little paramagnetic compared to a vacuum. Air should be 3·49 to be one fifth of the oxygen, and that would have required an experimental result of 43°·1 instd. of 44°·3 or 44°·6 (12823, 8). Still, if an error of experiment, it is not great—and a few averages will nearly remove it.

12845. The result with the copper is a very striking and good one.

12846. The mode of observing *between* the two stops is a very good one for cases of attraction.

12847. The arrangement (12843, 20) is like the former except that the former 2nd and 3rd column is replaced by one column having the intervals reduced to the centigrade scale of air and water, and then the third column is constructed from that by addition or subtraction.

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12848. The beam was left last night with the two weights on and suspended; the torsion index at 0·6 R and the place then 7·37 (12842); no extra iron about the poles. This morning the temperature was 57° F., and at 7^h 50′ the place was 13 or a little more, being 5·7 on the scale more out than when left last night. Then lighted the gas burner and in an hour, i.e. 8^h 50′, the place had gone back to 8·2—at 9^h 50′, its place was 7, or 7·1, and at 10^h 40′ the place was still 7·1 or 7. So that gas light from 7^h 50′ to 9^h 50′, or for 2 hours or less, had made the beam go from higher to lower numbers to the extent of 6 inches on telescope scale, 5·3 of which were in the first hour.

12849. Leaving the gas burning and other things the same, I then put the extra iron about the poles as usual, and 2½ hours after or at 1^h 15′ P.M., found the beam steady at place 8; as if the extra iron had allowed the beam to go *out* about 1 telescopic inch.

12850. Now put out the gas at 1^h 20′—and then the follng. changes happened—at 2^h 10′ or in 50 minutes, the place was 8·6, having increased—at 5^h 20′ or in 4 hours it was at 7—at 9^h or after 8 hours from the extinction it was still at 7.

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12851. Having been left all night, it was this morning at 7^h 45′ at place 6·25. Lighted the gas burner at this hour, and then at 7^h 55′ the place was 7·7, and it was moving unsteadily as if the expanding or warping wood did not give way readily. In five minutes more or at 8 o'clk., the place was 8·3—also at 8^h 45′, the place was 8·3—and at 9^h 25′ the place was still 8·3, so that it seems now steady under the circumstances of the gas light, etc. At this hour or 9^h 25′, I took off the extra iron, which of course shook the box somewhat, and then left it until 10 o'clk., when place

was 8—at 10^h 30′ place was 7·6. Considered the beam and wire steady and concluded these observations:

At the end of a day's work (12842) the place was	7·37	
After a night of 15 hours, no extra irons or gas	13	gone out during the night.
Then gas for two hours—no iron	7·1	Gas light rapidly took end in.
Then irons away and gas still on for $2\frac{1}{2}$ hours	8	Extra iron on—end goes out.
Then putting out gas light—eight hours after	7	light extinguished— end goes in.
A night of $10\frac{3}{4}$ hours more in same state	6·25	
Then gas for an hour (2 hours did no more)	8·3	Gas light—end goes out.
Then Extra irons off and 1 hour after, steady at	7·6	Extra iron off, end in.

12852. Perhaps the first great effect in the mrng. is due to change in the temperature of the place. Requires a couple of hours of gas light, etc. to get the beam right.

12853. Made the Zero 0° and then the place was 6·6—$\overset{5\cdot7}{4\cdot8}$—etc. etc.—or 5·7. *Now* 0°·1 *at the index is equal to* 0·3 *in the telescope place*—so that Zero is really at 0°·1 R torsion when telescope place is 6, and it is essential that the telescope place should not change or be changed.

12854. Proceeded to observe, using a new vessel. I have made several, larger than the former, of one piece of thin glass tube, and four of these I have prepared and sealed up as Vacua by means of the air pump. They are numbered V, VI, VII and VIII, and their weights empty are

V . . 106 grains—full of water 225 gr. water = 119, and capacity is 0·472 c: i:
VI . . 118·5 gr. full of water=
VII . . 115·5 gr. full of water=
VIII . . 121·5 gr. full of water=

12855. The following series is with
Vessel V—distance 0·5—Temperature 57° F. Zero torsion 0°·1 R.

12856. *Air Vacuum*—169°—170°+0°·1=**170°·1**. On breaking open the end the sound of the entering air was [as] if a good vacuum had existed.

12857. *Air*—151°—150°·8=**150°·9**.

12858. *Oxygen*—81°·5—81°·7=**81°·8.**

12859. *Carb. Acid gas*—169°—170°·2= **170°·3.** On adding some Nitrous gas to the Carbonic acid gas in the jar, no tint as of oxygen appeared—no oxygen here.

12860. *Nitrogen*—freshly made by plenty of burning phosphorus and then left over water 2 hours about—165°·5= **165·6.** The addition of a little Nitrous gas shewed oxygen in the gas left in the jars. So when the yellow tint was washed out—tried the remaining Nitrogen; now it was 167°—166°·6 = **166°·7**—so higher than before. A little more nitric oxide gas again shewed orange fumes—washed these out and then tried the nitrogen left, which was 165°—167°= **167·1.** This is more diamagnetic than the former two—added a little more N. Gas, which shewed faint red fume. After this was washed out, the remaining nitrogen gave 167°·2—167°·1=**167°·2.** This last gas contained a little excess of N. Gas and was faintly reddened by the addition of a little air. It must have been very close upon pure Nitrogen—the force of which therefore may be taken as 167°·2 experimentally.

12861. Nitrogen is clearly less diamagnetic than Carbonic acid Gas and stands above it in my order. It is less diamagnetic than the Vacuum in the prop. of 167°·2 to 170°·1, as if it were really above the Vacuum in the order and *paramagnetic* in itself. Must correct or confirm these results when working with a C.A. Gas vacuum or a hydrogen vacuum.

12862. *Water*—637°, i.e. one revolution+277°. The vessel filled with water weighed 225 grains—hence its capacity is 0·472 of c: i: With 2 weights on the beam, I gave the reverse torsion of 637° and then, placing the index at Zero, left the beam to take its place.

6·35 6·25

This was successively 7·5—5·2—7·3—or 6·25; so it is nearly as it was at first, i.e. at 0°·1 D now, being before at 0°·1 R. The reversion has taken out any set it might have at the moment of observation, which probably was not more than 2° or 3° at the utmost.

12863. Now left this series with vessel V, which gives numbers with water and like bodies too high at this distance, and returned to vessel IV. So

Vessel IV—*distance* 0·5—*Temperature* 58° F. *Zero of torsion* 0°·1 D.

12864. *Water*—329°=**328°·9.**

12865. *Alum solution* No. 1 (12761, 832)—330°·5—332° = **331°·9.**
12866. *Alum solution* No. 2 (12773, 833)—329°·4—331° = **330°·9.**
These three were taken with great care and the results shew how near they are together. Many of the discrepancies of former results are due, I think, to my having gone to work too soon after lighting the gas—and carelessness about the doors and cold draughts of air.
12867. I have a colourless solution of oxide of copper in ammonia, made by adding excess of ammonia to solution of proto muriate and then introducing bright copper wire and leaving the whole for some months. It is perfectly clear and when opened smells strongly of ammonia, but there is protoxide at the bottom so that it is saturated. This solution gave the following results:
12868. *Prot. amm. sol. copper*—326°·5—326°·1 = **326°.** Then put the clear solution into a bottle large and with plenty of air and agitated it well. It became a deep blue but deposited nothing, all the copper remaining dissolved. This solution
12869. or *blue amm. sol. copper*—283°—285° = **284°·9.** So the substance has become less diamagnetic by the oxidation of the copper (12895).
12870. *Sol. of Manganate of Potassa*—very clear and old and of a fine deep red colour—335° = 334°·9.
12871. *Ammonio sul. of Nickel.* A solution of ammonia same as before (12831) had crystals of Sul. Nickel added and dissolved until a little deposit, grey, was produced, and was then filtered. Its strength I do not know. It was attracted in the vessel IV, and with stops up on each side of place 6 (12830, 46) with a force above 3 revolutions or 1080°. Stopped the experiment.
12872. Strong pure Sulc. Acid, i.e. without iron—353° = 352°·9.
12873. *Fluid chloride of Arsenic—dry*, not hydrated—402° = 401°·9—is high as a fluid diamagnetic body.
12874. *Hard stearine from hog's lard* (12837), whilst in the fluid state and at the temperature of 98° F. or 100° F.—its torsion force was 279° = **278°·9.** Being left for an hour at the magnet, whilst the beam was on the fork and the torsion off the wire (during tea time), it was afterwds. when cold and solid at 300°·6 = **300°·5.** So there is a serious difference here; is it due to the difference of solid and fluid, or to difference of temperature between 98° and 58° F.? The stearine has contracted in cooling and has cracked

within and has also in places separated from contact with the glass, so that probably a little air has entered as the bottom aperture is not closed—but that ought to make it less diamagnetic, not more so. If the spaces were vacua also, still the whole ought to be less diamagnetic. It would be curious if the same weight of a substance, whilst it became less in bulk, should become more diamagnetic in force. The difference is more probably due to the two states of solid and fluid. Can it have had a structure given to it at the Magnet?

12875. Here the neck of the vessel broke and thus it came to a termination. Temperature is now 58° F.

12876. With 2 weights on the beam and the index at 0°, the
5·35 5·3 5·45
places were after a time at 7·1—3·6—7—3·9. So the Zero is now about 0°·2R (12853). Then put the extra iron at the poles, and left the beam as it was all night.

See over for the tabulated results[1].

12877. *Series Eight*—dist. 0·5—Temp. 57° F.—Vessel V—capacity 0·472 c. i.

	Exp. results	W. and Air, centigrade	Vacuum Zero
Oxygen	81°·8	16·82	18·17
Air	150°·9	31·04	3·95
Nitrogen	167°·2	34·39	0·60
Air vacuum	170°·1	34·99	0·00
Carb. Acid Gas	170°·3	35·03	0·04
Water	637°·0	131·04	96·05

12878. *Series nine*—distance 0·5—Temp. 58° F.—Vessel IV—capacity 0·2892 of cubic inch.

Amm. sol. Nickel—paramagnetic			
Prot. Amm. Copper	326° attr.	114·6 att.	134·23
Per Amm. Copper	284°·9 attr.	100·2 att.	119·83
Air from (12843)	44°·6	15·68	3·95
Air vacuum deduced		19·63	0·0
Stearine fluid	278°·9	98·1	78·47
Stearine solid	300°·5	105·6	85·97
Water	328°·9	115·68	96·05
Alum Sol. No. 1	331°·9	116·7	97·07
Alum Sol. No. 2	330°·9	116·4	96·77
Per Manganate potassa	334°·9	117·8	98·17
Pure Sulc. acid	352°·9	124·1	104·47
Chloride Arsenic	401°·9	141·36	121·73

[1] I.e. par. 12877.

12879. The results with vessel V (12877), which is large, were carefully obtnd. and are very good, and I think the gases will stand as placed. Perhaps Nitrogen is paramagnetic by a little oxygen or N. Gas present—and perhaps of itself. If it were and at 0·3, it would then give the oxygen five times the oxygen in air. C.A. Gas was clearly more diamagnetic than the nitrogen in the expts.

12880. The results of the ninth series shew clearly the paramagnetic character of the copper compounds and that the protoxide is more so than the peroxide. The difference between solid and fluid stearine from hogs' lard is also striking—and the high diamagnetic character of Sulphuric acid and the chloride of Arsenic is also very interesting.

30 OCTR. 1852. GREAT LOGEMAN MAGNET.

12881. Found the beam this morng. at place 2·7—on Thursday morning (12848) it was found at 13—or quite the other side of 6. To-day the gas was lighted at $7^h\ 50'$, and immediately the place in the telescope *increased* and both gradually and steadily—in 7 minutes it had become 5·3—oscillated a little to 5·1 and then went on, so that at 8 o'clk. it was 5·7—and then swung back to 5·3 or 5·4 very slowly. At $9^h\ 10'$ it was steady and its place 5·6, being very little removed from the normal position and state of torsion.

12882. I now took off the extra irons from the poles, leaving every thing else unchanged. Returning at 11 h. 35′, or 2 hours 25 minutes after, the place was steady and at 5·4, so that taking off the irons seemed to take the object in 0·2, but the change is very small. Put the Index back to $0^\circ 2$ R on the scale, making that the Zero of torsion with place 6 in telescope.

12883. Worked with a new vessel, being one of the I, II, III, IV series (12768) and of the same tube—will call it No. IX—its weight full of air is 75 grains—full of water is 146·6 grains—so the water = 71·6 grains and the capacity is 0·2836 of a cubic inch.

Vessel IX—Distance 0·5—Zero $0^\circ{\cdot}2$ R—Temp. $59^\circ{\cdot}2$ F.

12884. *Water* first—$365^\circ{\cdot}5 - 368^\circ + 0^\circ{\cdot}2 = \mathbf{368^\circ{\cdot}2}$.

12885. *Potassa caustic*, a strong solution made last night and

now clear by standing—417°—414° = **414°·2**; is a high diamagnetic.
12886. *Water* again—370° = **370°·2.**
12887. Then balanced the beam with its two weights and placing the torsion index at 0°, left it half an hour and then found the successive places to be 8—4·46—7·4—4·64—7·0—4·8. So the place is 5·8 or 5·9, and the Zero close upon 0°—certainly not 0°·1 R.
(6·23 5·93 6·02 5·82 5·9)
12888. *Air* in the vessel—54°·7—**55°·5**.
12889. Now proceeded to fusible bodies, putting the substance into a tube of sufficient length, fusing it, immersing the bulb and part of the upper neck of the vessel IX, both ends being open; and when the bulb was full, sealing the upper end and withdrawing the vessel to experiment with it. After being wiped—brushed with the damp camel hair brush for electricity—and placed quickly on the balance—it was observed as soon as possible, while yet warm and partly liquid, and then left to solidify and cool and observed again.
12890. *Spermaceti*—319°—it had begun to crystallize at the top and bottom in the necks, the temperature being about 111°·5 F., as was ascertained by another experiment with the same spermaceti. Its telescopic place did not change as the crystallization went on but remained for some time the same. After a while, the beam was taken up on the fork—the vessel left in its place close to the magnet—the torsion on the wire of 319° undone and reversed and the index left at Zero, and all allowed to cool. At the end of $1\frac{1}{4}$ hours, when the temperature had sank to about 60° F., the torsion force for place 6 was 330°, being far more than before. When the torsion was made only 319°, the object was out between 6·1 and 6·2.
12891. *Camphor*. This not so good a substance as spermaceti, for it contracts very much whilst crystallizing and crystallizes soon, so that when first placed on the beam, some air—a bubble of this size—was in and much camphor was solid; but it was hot, nearly up to the fuzing point throughout, which is 380° F.

At first it was fairly 308°. As it cooled, the object went out and soon required 323°·6 torsion for place 6—being left a little longer, it was higher still and up to 327°. But I did not wait much, for when the whole was solid, I think nearly a fourth

must have been air or vacant space. Still, the effect is just as above. Cold solid camphor must be more than 327° at distance of 0·5.

12892. *Wax*—a specimen of pure bleached African wax—very good. This was a substance easily managed—it contracted on solidifying and cooling, but not so much as either camphor or spermaceti. Whilst hot, but a little of it solid, and having a temperature of 145° F., it required a torsion of 306° for place 6. Gradually a higher torsion was required—first 321°— — —then after different intervals 325°— — —335°— — —340°— — —. It remained steady at 340°, having attained the common temperature of .

12893. I took off the object—balanced the beam with two ring weights—did not reverse the torsion, but placed the index at 0° to see what set might have done in any part of this experiment.

6·45

The successive extreme places were 12—0·9, and in half an hour's time the beam was steady at place 5·75—so that there was very little set indeed, for the torsion is almost as at the beginning, that is Zero is 0°, or less than $0^\circ{\cdot}1$ R (12887).

12894. I have left No. IX full of wax to experiment with hereafter in respect of the action of heat; i.e. after I have seen what happens to oxygen and air by a cold bath.

12895. No. IV vessel was filled with the solution of Ammonio Mur. copper (12868, 9) and then the liquid evaporated; it yielded a dry salt, deliquescent, but which when dry weighed 0·9 of a grain. So small is the proportion of copper present.

12896. In the experiments with spermaceti, camphor and wax, the results may be due to change of state or change of temperature. Those with the spermaceti seem to shew it is not change of state, for it did not change whilst solidifying but whilst *cooling*. As to the cooling, whilst the body is warm, a current of air will ascend from it and so lateral air go in towards it and, this being obstructed on the magnet side, may have been more abundant on the other or open side, and so have carried the object inwds.; and this would account for the effect—for the colder the body became the more it would go out. Must ascertain the influence of this cause by having a block on the outside of the same form as the magnet.

12897. The last results, adjusted in a series, give the under view: Series X—distance 0·5—Temp. 59·2 F.—Vessel IX—its capacity 0·2836 of c: i:

	Exp.	W. and Air = 100	Vacuum Zero
Air	55·5	17·7	3·95
Zero Vacuum deduced		21·65	0·0
Camphor hot	308°	98·1	76·45
Camphor cold	327°	104·24	82·59
Wax hot	306°	97·55	75·9
Wax cold	340°	108·38	86·73
Spermaceti hot	319°	101·7	80·05
Spermaceti cold	330°	105·2	83·55
Water	368°·2	117·3	95·65
Water	370°·2	118	96·35
Potassa solution	414°·2	132	110·35

12898. 1st Novr.—Monday Evng.—the beam as left (12893) on Saturday night was now at place 4·95.

12899. 2nd Novr.—Tuesday at 1 P.M.—place 5·25—moving about made it 5·0. Lighted the gas over it at 1^h 12′—immediately altered—the place became 4 in four minutes—at 1^h 21′ or in nine minutes, was at 3·4—and at 1^h 30′ the place was 3. It did not appear likely to go beyond this and I did not wait for any thing further.

12900*. Have made a wooden block to fit into the Magnetic field angle so as to cause as much obstruction to the motion of air on the one side as on the other (12896). The form is as in the sketch, where M is the magnet or its poles, W the wooden block, B the glass cylinder or bubble, and S the space around it. The block W is as deep as the poles and is supported on a slight leaden leg attached at L, so that the motion of the air should not be obstructed. B, when in place 6, stands in the middle of the prismatic space S.

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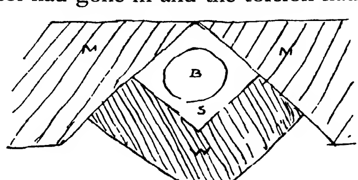
12901. No. IX vessel, containing Wax (12894). Had the wax melted and then was hung on the balance, all being in order. The torsion was 287°·5—285° for place 6. This was at 3^h 57′ and the wax was solidifying—at 3^h 60′ the place was the same. At 4^h 10′ the object had gone in and the torsion had to be reduced

* [12900]



to $277^\circ{\cdot}5$ – again the like effect and at $4^h\ 15'$ had to reduce torsion to $272^\circ{\cdot}5$. At $4^h\ 52'$ the necessary torsion had sunk to 266°. It did not seem to go lower but being left until after tea, i.e. for an hour, the torsion then was the same, i.e. 266°. I then removed the wooden block W and still the torsion remained 266°. So that when at the temp. of the atmosphere, the torsion is the same whether the block is there or away.

12902. Now reheated the wax and object No. IX and put it on the beam hot, the block W *being away*. The torsion for place 6 was only 243° – all the wax being fluid. On putting up the block W, the vibrations were between 6 and 8, so that putting up the block made the object go out. Took away the block and then the vibrations were between 4·7 and 8, so the place was again near 6 or a little out, as at 6·3. When the block is there, the object goes out; when the block is away, it goes in. Being without the block, the place 6 now required 250° of torsion, or *more* than before – the wax is now solid but warm. After a time the requisite torsion was 254° – – – and again after a time 260° and still rising a little. I doubt not it would have come to 266° as above but I could not wait.

12903. So when the block is not in place, the cooling wax appears to become more diamagnetic than it was before when hot; and when the block is in place, the cooling wax appears to become less diamagnetic. I do not doubt that the first effect is only apparent and is due to the ingress of air towards the object in the angle to supply the place of the warm ascending current. In the second case, where the object is surrounded equably on all sides, this effect cannot happen, and it would appear as if the wax were *less diamagnetic when cold than when hot*. Before coming to a conclusion on this point, I must ascertain what effect, and to what extent, is produced by the warming of the oxygen of the air round the warm body. As the air is made less paramagnetic by heat, so the warm body and it together should be more diamagnetic than the cold body and investing cold air, even though the body were to remain unchanged.

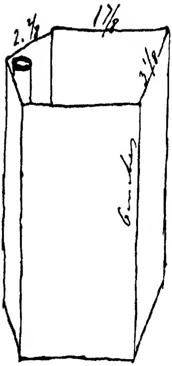
12904. Put IX filled with wax away as before (12894).

12905. I opened the tube round the wire and passed the feather brushes over it. Brushed out the box carefully. Made the distance

0·9 and adjusted it so that it should be right for the cold bath, Object No. 1 oxygen and place 6, and if needful, the stops. Then put the Extra iron round the poles, balanced the beam light with two weights, left it swinging—the gas burner extinguished and the balance covered with the cloth.

12906. I have filled tube I (12768) with oxygen, and also tube V (12854). I have dried out tube III and left it filled with air (12768).

12907. The smallest difference in distance affects the numbers obtained for the torsion, so that only those results which are in a series can be compared together, until they are reduced. Putting on and off of the extra iron disturbs the Magnetic poles in position so much that different series made at the same general distance of 0·5 may not be put together until reduced. Water for instance will come out differently in two or three different series all at 0·5 distance, and so will air and oxygen, but when reduced they appear beautifully together.



3RD NOVR. 1852.

12908. A cell and cold bath have been arranged. The bath is of thin sheet copper of the size given–the angle goes up into the angle of the poles in the magnetic field. The cell is a thin copper tube as high as the bath and $\frac{7}{8}$ of an inch in diameter, fixed in the angle upon the bottom of the bath within, so that it should be near up into the angle of the magnetic field and yet that the frigorific mixture to be used in the bath should circulate freely round it. The large vessel therefore is a cold bath of 48 or 49 cubic inches capacity to the small air cell, and the air cell is a cold bath to the objects hung upon the balance and immersed in the air of the cell. The expectation is that the bodies will be cooled; that the cold air in the cell will continue in the cell, giving an undisturbed unchanging atmosphere, and that the cold air of the cell and the cold object in the air may be compared with each other magnetically. The nearest distance at which I could place this bath gave me a distance of 0·9 for the object center or thereabouts. The bath was invested with a jacket formed of three folds of dry flannel.

12909. This bath was filled to within $\frac{1}{3}$ of an inch of the top, equal to 44 cubic inches, with a good mixture of salt and pounded ice, and put into its place at the magnet, after it had been ascertained that with place at 6 the Zero was at 5°D. The external temperature was 60°·5 F. Vessels Nos. I, III and V were ready to work with (12906). No. I is filled with *oxygen* and has a short hook. No. III has a hook a little longer and is filled with *air*. No. V is a tube of larger diameter with a long hook and is filled with *oxygen*.

12910. No. III tube (12768) filled with air and open at both ends, being on the beam and in the cell, has room to vibrate freely on both sides of place 6, in fact between 3 and 8. (Distance is 0·9–telescopic place is 6–Zero of torsion is 5°D). At 10^h 9′, with direct torsion of 14°, its place was 6 or 6·1. At 10^h 15′–torsion was 15°–place not quite 6– – –with 16°, place about 5·4– – –with 15°·5, place about 5·9 at 10^h 22′; but the motion is very slow and it seems as if the middle of the cell is a place of unstable

equilibrium, very weak however in character; probably the object is not yet quite at the temperature of the cell and so there are currents of air inside of it.

12911. Then after a little while longer, place became very steady at 6 for torsion of 15°·5 or 15°·4. Being left for half an hour longer, it remained the same. Hence 15°·4 minus 5° for correction gives the torsion (direct) as 10°·4 for this object. Hence the cold glass is diamagnetic in cold air to this amount at this distance of 0·9 and at the temperature (assumed) of 0° F.

12912. Took the beam up on the fork—took off No. III vessel, which was very cold and instantly became dim from moisture condensed upon it from the air. Put on No. I tube (12768, 906), very nearly the same size and substance as the last but containing *oxygen gas*, keeping it in the center of the cell until cooled by not letting the beam off the fork. Brought up the stops also on each side of place 6, balanced the object on the beam and then left the beam suspended for a while (and stopped) to cool. At $10^h\ 45'$, the torsion index being at 5° D, the place was 6—the bath was in a good state and salt on the top of the upper ice—the beam swung very well and freely between the two stops. At $11^h\ 0'$, the beam was very steady and the condition of things apparently settled. Made the 5°—4°·5, and now the object kept constantly outside of place 6, yet vibrating in a manner to shew it was very free and nearly right. Then it went very leisurely inside of place 6—perhaps feeble currents in the cell may a little affect it, being as it is so near to the Zero of torsion—then outside of 6 again. I consider between 4°·8 and 5° as very nearly right, which being corrected gives 0°·2 R or 0°·0, as the torsion power for this object.

12913. There was an ink mark | on the vessel during this time on both sides—removed this and then restored the vessel to the beam and bath at $11^h\ 10'$—left it to cool and settle—at $11^h\ 20'$ it tended with the last torsion to go in rather. Made torsion 4° D and now the object clearly tends to go out. The torsion is I think when all is cold at 4°·6D. So the ink mark not sensible—and the real torsion after correction 0°·4R.

12914. Now stirred up the bath, which was still in excellent condition with plenty of ice at top and salt on that, and then put No. V (12854, 906) on the balance. This tube is filled with

oxygen and is larger than the former, but found there was room enough for free vibration. As a whole it is diamagnetic at this low temperature—the torsion appeared at successive periods to be $11^\circ{\cdot}5$—12°— —13°— — —14°—this too much—$13^\circ{\cdot}5$ a little too much—13° gives place 6·05 at 11^h 45′—at 12 o'clk. it had gone in a little and 13° was too much—with $12^\circ{\cdot}5$ torsion it was too little. Finally adopted $12^\circ{\cdot}8$ direct torsion as correct, which being corrected gives $7^\circ{\cdot}8$ as the true amount.

12915. On taking out this object, I put a small spirit thermometer into the cell with its bulb not touching the side but in the place of the object V. It gradually became 32° F.—28°—25°—22°—20°—16°. Then stirred the frigorific mixture, but the ice and the salt nearly all dissolved—still the temperature of the air of the cell went down directly to 15° and after a little while to 13°— — and 12°. At this moment the thermometer was transferred from the air in the cell to the liquid in the bath, and that found to be at 10°. Hence it is evident that the air in the cell quickly changes in temperature with the liquid in the bath—and that finally there is not more than a difference of 2° between them.

12916. Any current in the balance box from the descent of cold air about the bath would tend to carry the beam above outwards, because of its position, i.e. farther away from the magnet. If any current existed in the air cell, it would probably tend also to set the object outwards, because the outer side is that which is in freest contact with the freezing mixture (12908). But I expect that after a time neither of these causes has any sensible, or at least only a small, influence.

12917. Now took away the bath, opened the top and door of the balance box to let cold air out freely. Then restored the last object, No. V, *oxygen*, to the beam in the air at common temperature, putting a thermometer bulb close beside it. This thermometer shewed $59^\circ{\cdot}5$ F.—and the torsions requisite for place 6 were $12^\circ{\cdot}5$— —$12^\circ{\cdot}4$— —$12^\circ{\cdot}3$—the latter is too little— —$12^\circ{\cdot}5$ not quite enough—$12^\circ{\cdot}6$ not quite enough— —$12^\circ{\cdot}7$ seems to be becoming more diamagnetic— —$12^\circ{\cdot}9$— —temperature now 60° F.—$13^\circ{\cdot}4$ not too much— — —put a spirit lamp for a moment or two inside the box—the temperature now $61^\circ{\cdot}4$ F—the torsion 14° and that apparently not too much—left it a little while—think it is too

much and that 13°·8D is about right. This corrected gives 8°·8 as the torsion.

12918. Placed No. I Oxygen on the beam at this distance and a temperature of 61°·4 F.—5° D is too much—4°·5 is too much—4° is about right—is a little too much. The object is very near to Zero. The vibrations appear to be about equal in force on the two opposite sides of 6 and so conclude that 4° D is correct. This gives the torsion as 1° R.

12919. Placed No. III or air on at the temperature of 62° F. The torsion was 14°— —13°·5—after dinner the place was 6·5 with the 13°·5 torsion direct— — —torsion 13°·7— — —14°—close to right, but left it on and torsion became 14°·1, which being corrected becomes 9°·1 D.

12920. Then balanced the beam with 2 weights on, and left it to settle with the torsion index at 5° D—after a while the following

6·62 6·47 6·5

extreme places were obtained—7·5 5·75 7·2 5·8. So 6·5 is about the place, and the true Zero for place 6 is close upon 5°·2 D.

12921. No. V and No. I were both well sealed and apparently in good order. No. V had a little dampness inside, probably from the oxygen having been put through and in from over water.

12922. As to No. III, i.e. the glass tube containing only air and open; here of course it is the glass which might be expected to give its own particular effect in the same surrounding medium of air. When warm the torsion was 9°·1,
when cold (0° nearly) „ 10°·4. So that the glass by cooling seemed to increase in diamagnetic power by 1°·3. But it may be that the air around it increased in paramagnetic force to the same amount of 1°·3. This concerns only the solid bulk of the glass and not its bulk as a vessel. The glass weighed only 81 grains (12768).

12923. No. I, oxygen, was a like vessel of the same glass (12716); its weight was 92 grains and it was somewhat larger than the last; the capacity of No. I being 0·384 of a cubic inch and that of No. III 0·309 of a cubic inch. When warm the torsion was 1° R.
When cooled to 0° F. „ „ 0°·0 or 0°·4 R.
So the cooling appears to have raised the glass a little in diamagnetic power, i.e. by 1° or 0°·6, or else to have diminished its

paramagnetic power by the same quantities of 1° or $0^\circ{\cdot}6$. But the contents are oxygen and the expectation was that this oxygen would have become more paramagnetic by cooling, not less so. No doubt the air would also change, i.e. the oxygen in it, but unless the nitrogen has also changed in some degree, the result does not seem consistent. It is however true that the glass alone (12922) appeared to become $1^\circ{\cdot}3$ more diamagnetic by cooling, and as this glass is to the former as 9 to 8 by weight, so we may allow for it roughly $1^\circ{\cdot}46$ for increase of diamagnetism. This in place of leaving the oxygen 1° or $0^\circ{\cdot}6$ extra diamagnetic would indicate that it was $0^\circ{\cdot}46$ or $0^\circ{\cdot}86$ extra paramagnetic.

12924. Viewing it another way—here are the results and the differences.

		warm	cold	
No. I.	Oxygen	$1^\circ{\cdot}0$R	$0^\circ{\cdot}0$–	$-0^\circ{\cdot}4$R
No. III.	Air	$9{\cdot}1$D	$10{\cdot}4$D	$10{\cdot}4$D
	difference	$10^\circ{\cdot}1$	$10^\circ{\cdot}4$	$10^\circ{\cdot}8$

So then *warm* oxygen is paramagnetic to air by $10^\circ{\cdot}1$ in respect of the present volumes—but when cold, by $10^\circ{\cdot}4$ or $10^\circ{\cdot}8$. As the glass is cooled in both cases, that has not to be considered in these rough comparisons; but again the difference if hot or cold seems very small, smaller than I expected.

12925. Cold appears to make glass more diamagnetic than it was when warm (12922). On the contrary, on a former occasion (12903), Wax seemed to become less diamagnetic by cold. There may be this opposite effect but it is unlikely; and again it appears to me possible that in the case of the glass the effect may have been due to increased paramagnetic power of the air (12923). But then this could not be of the oxygen of the air only, for then we should have had a greater difference above. Is it not possible *that at low temperatures the nitrogen also has become paramagnetic?* Must ascertain this point next.

12926. No. V oxygen gave torsion of $8^\circ{\cdot}8$D when warm and $7^\circ{\cdot}8$D when cold, the difference being 1° more paramagnetic when cold than when warm. If the cold glass, weighing 106 grains, be assumed here as extra diamagnetic by cold to the extent of $1^\circ{\cdot}5$ (12922), then the cold oxygen would appear to be extra

paramagnetic by cold 2°·5. In the comparison of air and oxygen above at common temperature (12924), the 10°·1 may serve to represent the force of $\frac{4}{5}$ of the oxygen present, the other fifth being compensated for by the oxygen in the opposed or contrasted air. So that probably oxygen and nitrogen in about these bulks would at common temperatures be about as 12°·6 and 0°·0. This tube V is much larger, its capacity being 0·472 of a cubic inch, whilst I has capacity of 0·384, and III of 0·309 of a cubic inch. It seems to increase in paramagnetic power 2°·5–and a volume of ·347 of c.i. would at that rate increase in force 1°·84. In which view, that volume of oxygen would be paramagnetic with a force of 12°·6 at 62° F. and 14°·44 at the temperature of 0° F. Must clear up all this by further experiments and especially the effects of cold on glass (12922, 5).

12927. Both the *oxygens*, i.e. No. I and No. V, are less diamagnetic or more paramagnetic when cold than when hot. No. III with air is the contrary. The general results are as below:

No.		c. inch	62° F.	0° F.		
III	capacity	0·309–Air	9°·1 D	10°·4D	–increase	1°·3
I	,,	0·384–Oxygen	1°·0 R	0°·0	dimin.	1°
				0°·4 R	,,	0°·6
V	,,	0·472–Oxygen	8°·8 D	7°·8 D	,,	1°·

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12928. Found the beam this morng. at place 7·8, Index being at 5°·2D (12920). Left the cloth covering over the balance and lighted the gas; place soon changed to 7. When the cloth was quite off, it went steadily to place 6 in a few minutes. Two hours after it was at 6·2, so that no sensible change from the day before yesterday.

12929. I have made a third set of tubes for the balance from one piece of glass, intermediate in diameter to the two former. I have marked all those of the second and third set horizontally thus :- ☰ with ink to recognize them. The other numbers by which they are recorded in the experiments are as under:

12930.

first set	*second set*	*third set*
I. weight 92 gr.— capacity 0·384 of cubic inch	V. — — —106 gr. 0·472 c: i:	XII. *employed*—92·4 gr. —0·3644 c: i:
II. used up—88·5 gr.—0·346	VI. *vacuum*—118·5—	XIII. *employed*—104·6 gr. —0·4 of c: i:
III. — — —81 gr.—0·309		
IV. used up—81 gr.—0·2892	VII. *vacuum*—115·5—	XIV. *vacuum*—98 gr.—
IX. *wax*[1] —75 gr.—0·2836	VIII. *vacuum*—121·5—	XV. *air*—99·8 gr.—
X. *unused*		
XI. *unused*	*Several others*	XVI. *vacuum*—101 gr.—
No more		*Several others*

The tubes XII and XIII were prepared for cold experiments to-morrow.

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12931. Experimented with cold bath on certain gases, as Oxygen, Air, Nitrogen, etc. The bath as before at 0° F (12908, 9). The tube No. XII of the ≡ set (12930) weighed as a vacuum 92·4 grains, and afterwds. when full of water 184·5 gr.; hence the water is 92·1 gr. and the capacity is 0·3644 of a cubic inch. The distance was 0·9 as before (12908). The external temperature 59° F. The Zero of index 9°·5 D when the telescope place was 6, the irons being off and the gas having been alight some time. The tube No. XII has been filled with Carbonic acid gas—then exhausted by the air pump and sealed up; so is as a Carbonic acid gas vacuum.

12932. *Carbonic Acid gas Vacuum.* In air at common temperature of 59° F—24°·5 torsion—24°—24°·1. This minus 9°·5 for correction to true Zero (12931) gives 14°·6 as the direction torsion required by this vessel at this distance and state.

12933. The cold bath was introduced in a good state—may consider it and its cell at 0° F. The beam could vibrate between places 3 and 8·5, there being plenty of room for the vessel. Now the Carbonic acid gas vacuum gave the following numbers, for I left it a good while, 40 minutes or more, before the experiment was concluded, that the vessel might be thoroughly cooled down, but observed during the time to see what differences appeared. Torsion was at 18°·5, but was vibrating— — —20°— — —22°— — —

[1] Words in this table in italics are in pencil in the MS.

22°·5— — —23°; beam very nearly at rest and this 23° clearly too much as yet— —22°·5— —22°·6— —22°·7— — —23° again— —23°·5. Here I think it settled and I took this as the number, which being corrected gives 14° as the torsion.

12934. Part of the variation above is due I think to the currents of air in the cell during the cooling of the body. As it cooled, an ascending current would pass from it and a descending current would occur against the inside of the air cell. This probably would be a little more powerful on the side away from the angle, for that is the side most freely exposed to the cooling mixture, and such an effect would probably urge the object a little towards the opposite side or towards the angle. This I think is the reason why the 23°, being too much at first, is not too much at the end. What the current did whilst existing ceased when the current was over, and then the torsion did the whole and so gave a truer measure. If there were any current in the balance box generally, due to the cold bath, which would be sensible on the beam above, I think it would, from the position of all the objects, tend to carry the beam end out from the angle; but from the well clothed state of the bath, its low position, etc., I do not think such a current at the beam would exist. I count on the air in the cold cell being as a body quiescent because of its low temperature compared to the air above.

12935. So the cold glass and vacuum seems to be less diamagnetic than warm glass and vacuum. Assuming the warm and cold vacuum as being alike, cold glass appears to be less diamagnetic than warm glass. This is contrary to the former conclusion (12922), but compare these things presently.

12936. Opened the vessel XII at both ends and let *air* in—also sent *air* through. Now replaced the vessel in the cold cell. Then the torsion was in succession—20°, too much— —17°—18°, clearly too little—18°·5—19°—20°—20·3—20°·5. This last number, being corrected by 9°·5, gives 11° as the torsion for *cold air*. The difference between this and a vacuum being only 3° would seem to shew that Nitrogen could not be in any degree magnetic at this degree of low temperature. The change as the temperature fell was just as evident here as before; 20°, which was at first far too much, was not enough at last. I still refer it to the inner currents due to the cooling of the object.

12937. The same vessel XII filled as on former occasions (12716) with fresh good *Oxygen*. In the cold cell. The stops up, so as to restrain it on both sides of place 6 (12830), for the object is paramagnetic as a whole. 3°·5 D too little—4° too little—4°·5—5°—5°·5—6°—6°·5—7°—7°·5—8°. These are not gradual changes in the object, but appro[xi]mations to the place of an object having a position of *unstable equilibrium*. 8° was too much direct torsion and 7°·7 about right. This corrected gives 1°·8 R reverse torsion as the measure for the cold oxygen.

12938. The same vessel XII filled with *Carbonic acid gas*, in the cold bath: 23° too much—22° rather too little—place is about 6·1—by waiting, 22°·2 not quite enough—then 22°·5—23°—22°·8—22°·7, which corrected by 9°·5 is **13°·2**. The case still gradually creeps up as the low temperature is attained. I still attribute that to currents in the cell.

12939. The bath, being now examined by the thermometer, was at 10° F., so that Carbonic acid gas had probably not been quite so cold as oxygen or air. Prepared a fresh and excellent bath.

12940. Vessel XII filled with *Nitrogen*—22°·7 left by the C.A. experiment was too little—25° too much— — — —23°·5 not far off the truth; went to dinner for half an hour; on returning found the object close to 6, torsion 23°·5 or **23°·45** is right, which corrected is **13°·95**.

12941. Vessel XII filled with *Hydrogen*—23° a little too much at present—22°·5—too little—22°·7— — —23°·3— — —23°·5— — —24°— — —24°·5— — —settled at this number, which corrected is **15°**.

12942. Found the bath by the thermometer still at 0° F and in good state; removed it and opened the case a while to remove the cold air and coldness, and put a thermometer in with its bulb close to the place of the objects on the balance. Then proceeded to try the gases in the same vessel XII at common temperatures—and first the vessel as it is.

12943. Vessel XII filled with *hydrogen*, at temperature of 59° F. The torsion was 23°·5—22°—**21°·7**, which corrected is **12°·2**.

12944. In the mean time I had put the bulb of an alcohol thermometer into the middle of the cold cell, now removed from the balance, to see how much it fell and how quickly. It is but a small object and yet is massive compared to the gas bubble; it

descended to $25^\circ-20^\circ-$ —to 5° slowly, taking $10'$, $15'$ or $20'$ for the purpose. I then stirred up the contents of the bath and the thermometers in the cell gradually came to 0°. On the whole, I conclude the gas bubbles may be supposed to have sunk to about 5°, with the exception perhaps of the Carbonic acid gas charge. The vessels always shewed their coldness by suddenly condensing a film of hoar frost on their outsides when brought into the air.

12945. Vessel XII, filled with *common air*—at temperature of $59^\circ\cdot5$ F.—19°—$19^\circ\cdot1$, which corrected by $9\cdot5 = \mathbf{9^\circ\cdot6}$.

12946. Vessel XII, filled with *Oxygen gas*—$7^\circ\cdot2$—8°—$8^\circ\cdot5$—not quite enough—$8^\circ\cdot8$— — —$9^\circ\cdot2$— — —$9^\circ\cdot5$— — 10° very nearly right, I think—$10^\circ\cdot25$ too much—10°, which corrected is $0^\circ\cdot5$ D. The temperature then was 61° F.

12947. Vessel XII, filled with *Nitrogen gas*—$23^\circ\cdot5$—24°, which corrected by $9\cdot5$ is $\mathbf{14^\circ\cdot5}$—the temperature now being $61^\circ\cdot3$ F.

12948. Vessel XII, filled with *Carb. Acid gas*—26°—$25^\circ\cdot5$—25°—24°—$24^\circ\cdot2$, which corrected by $9^\circ\cdot5$ is $\mathbf{14^\circ\cdot7}$. Thermometer $61^\circ\cdot3$ F.

12949. Then placed the beam, with two weights on, in equilibrium as at first and with Index at $9^\circ\cdot5$ D (12931). When allowed to settle, it gave $5\cdot94$ as its telescopic place, which shews that the torsion is not sensibly altered and therefore the correction of $9^\circ\cdot5$ the only one needed in that respect.

12950. Vessel XII, filled with *water*, weighed $184\cdot5$ grains (12931). When on the beam at common temperature of $61^\circ\cdot5$ F., it gave torsion as $94^\circ\cdot7$—94°—$94^\circ\cdot5$—95°—96°, which last corrected is $\mathbf{86^\circ\cdot5}$.

12951. Now these in their cold para-diamagnetic order would stand thus[1]:

	cold	warm	temp. 62° F.
Oxygen	$1^\circ\cdot8$ R	$0^\circ\cdot5$ D	1° D
Air	11° D	$9^\circ\cdot6$	$\begin{cases}10^\circ\cdot8\\10^\circ\cdot8\end{cases}$
Carb. Acid Gas	$13^\circ\cdot2$	$14^\circ\cdot7$	$13^\circ\cdot8$
Nitrogen	$13^\circ\cdot95$	$14^\circ\cdot5$	$13^\circ\cdot5$
Vacuum C.A. Gas	14°	$14^\circ\cdot6$	
Hydrogen	15°	$12^\circ\cdot2$	$13^\circ\cdot5$
Water		$86^\circ\cdot5$	

[1] The results for Air and Hydrogen in the "warm" column are crossed through and a pencil comment "Bad results" is written against the table.

Here there is evidently something strange about hydrogen warm, also about Air warm and cold, as compared to oxygen. Can these have been influenced by cold currents in the neighbourhood of the magnetic poles? Hydrogen was the first and air the second tried after the removal of the cold bath. Before reasoning on the results, I shall repeat those in warm air with the same vessel and these gases.

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12952. Repeated all the above experiments at common temperatures, i.e. at 62° F, with the same vessel XII and at the same distance. The results therefore are comparable to or in place of those in the second column of figures (12951), but a paper cell of 2 thickness[es], being a tube of one inch diameter, contained the vessel whilst on the beam, so as to shelter it from currents. The effect of this tube was very good. The beam was at place 6 with Zero at 9°·5 D. The vessel XII (12931)—the distance 0·9—the temperature 62°—the gas had been several hours burning. The following is the order of the experiments.

12953. *Air* in XII—torsion 20°—20°·2—20°·3; this minus 9°·5 gives **10°·8**.

12954. *Hydrogen* in XII—23°, corrected = **13°·5**.

12955. *Oxygen* in XII. Stops up on each side of 6—9°·8 not quite enough, yet is almost, hangs at place 6·1—10°—not quite enough yet very near. Torsion of 10°·5—stands well at place 6—the object is not paramagnetic but very slightly diamagnetic; 10°·5 corrected by 9°·5 gives 1°D as the torsion for this object, oxygen, at this temperature.

12956. *Carbonic acid gas* in XII—20°—22°—23°·3, corrected by 9°·5 = **13°·8**.

12957. *Nitrogen.* Has been corrected by N. Gas—23°·3—23°, minus 9°·5 = **13°·5**. There was the slightest flavour of Nitrous acid produced in the mouth when the gas was drawn out of the bulb by the lungs.

12958. *Air* again in XII—20°·5—20°·3, corrected by 9°·5 = **10°·8**.

12959. Then the beam with the two weights upon it equipoised and left with Zero at 9°·5—settled at place 5°·92. So the torsion has remained unchanged during the experiments.

12960. All these results are entered in the third column of figures at (12951), and they shew the value of the sheltering cell (12952) and the inaccuracy of the second column of figures. The vacuum of C.A. Gas and the water are probably correct in the 2nd column, for the first was taken before the freezing mixture was used and the second at the very end of the expts. (12932, 50).

12961. It is not *sure* that the distances are the same for the 2nd and third columns, for the extra irons have [been] on and off between and the poles therefore a little shifted. The power in place 6 is apparently not quite so strong to-day as on Saturday. Still, I think it is clear that at common temperatures, Hydrogen, Carbonic acid gas and Nitrogen are close together. Unfortunately, I have the vacuum or water to-day. It is equally clear that hydrogen, Carb. Acid and Nitrogen are not together when cold (12951).

12962. It appears that Oxygen is more Magnetic by cold by between 2° and 3°, but the glass change has to be considered. That ought to be obtained from the air results, because the vacuum results give an unchanging space with a changing medium. From the results with air to-day and on Saturday, it seems probable that the glass does not sensibly change. Carbonic acid gas appears to become a little less diamagnetic by cold, but Hydrogen seems to be remarkable for a change in the contrary direction. It is as diamagnetic as Nitrogen or C.A. Gas at common temperatures; perhaps all are as a vacuum; but it is very clearly more diamagnetic than they are by cold.

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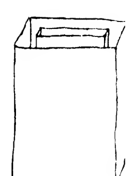
12963. Box brushed out ready for to-morrow's expts.

12964. Have prepared an outer Jacket of impervious caoutchouc paper to go over the flannel Jacket of the cold bath (12908), that the cold air in the flannel might be retained bodily against the outside of the bath. I know this is a good precaution.

12965. Have had a cool bath made for objects, consisting of an inner and an outer vessel attached to one bottom. Have prepared a flannel and an outer paper Jacket for it, as for the former bath (12908, 64).



12966. Have prepared a glass cell to use in air at common

temperatures (12952, 60) to keep off currents. It is an inch internal diameter, 6 inches high and attached to a leaden foot. It has been cleaned and then wiped inside and outside with a damp linen cloth to remove all electricity, and is now ready for careful use.

12967. Made some Nitrogen by putting air and Nitrous Gas together until the residual gas was not sensibly coloured (in a specimen) by either air or nitrous gas added to it; and then left it to stand over water as Nitrogen for tomorrow's experiments.

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12968. Cold Experiments on Gases. Zero 6°·6D—distance 0·9. Outer temperature, i.e. of the Room, 59°·7 F. The vessel No. XIII of the ≡ series (12930). It is now at the beginning a vacuum by the air pump and weighs 104·6 grains. When filled with water at a later period, it weighed 207 grains—the water contents therefore weighed 102·4 grains nearly, and the capacity is nearly 0·4 of a cubic inch.

12969. The glass cell (12966) in place. No. XIII *Vacuum* on the beam. It vibrates slower than if in the open air from the confinement of the air around the vessel and goes very little beyond its final place, but it takes time vibrating slowly. The torsion was 20°·3 D. Took the vessel XIII out of the cell, purposely touching its edge—passed the damp camel hair brush over it to remove electricity, if any—returned it, touching the edge of the cell—left it to settle in its place, which it soon did at 6·1 or less, in the same manner as before, the torsion being 20°·3 again. This corrected by 6°·6 gives for a *vacuum* **13°·7**. The temperature in the balance box was 60° F.

12970. Let in *Air*, and then ascertain the torsion under like circumstances—it was 17°·8, which corrected is **11°·2**.

12971. In order to judge what sort of vacuum had existed here and compare another set of results with these, I took vessel XV of the same ≡ set (12930), made a vacuum in the same way by the air pump, and ascertained its requisite torsion. 20°·6— —20°·8, which being corrected becomes **14°·2**. It was then opened under water and filled exceedingly well—after which it was emptied dry, left filled with air and put on the balance, when it required at the common temperature of 60° F., 18°·3— —18°·2, which being cor-

rected becomes 11°·6. So the difference here between air and vacuum is 2°·6, and the difference with No. XIII is 2°·5. The numbers corroborate each other and I conclude that XIII was a pretty good vacuum.

12972. Proceeded with effects at common temperatures first; and now *Carbonic acid Gas* in XIII, at temp. of 60° F.–20°–20°·4–20°·7–21°, which corrected equals 14°·4.

12973. *Nitrogen*, containing a trace of Nitrous vapour; 21°–too much–seemed to be clearly below the previous C.A. Gas–20°·5–20°·7–20°·9, which corrected is 14·3.

12974. *Hydrogen*–20°·9. The bulb settled exactly in the same place as the Nitrogen did for the same torsion–it and Nitrogen are alike at 60° F.–20°·9 corrected becomes 14°·3.

12975. *Oxygen*, fresh. Torsion being 6°·6, the bulb went out, so that as a whole it is diamagnetic. 7°–7°·5–8°·1–8°·5–8°·3–8°·4, which corrected becomes 1°·8. The temperature is now 60°·3 F.

12976. Deferred *water* at common temperature, and now took the same charge of oxygen (12975) cold. The bath had its paper cover and I poured off the liquid and filled up with mixed salt and ice, to have it in as good a state as possible. It was very cold the whole time of observation of oxygen; I have no doubt at 0°.

12977. The *Oxygen* vessel XIII was put into the cell at 12^h 25′ and remained in until 1 o'clk., or for 35′. The torsion, being at 4°D, required to be increased–4°·5–5°–5°·5–and this seems very near–6°·0 very close but not quite enough–6°·5 clearly too much at present–6°·3 appears to be exceedingly near, stands or almost stands at place 6. Has been in now 30 minutes. It moves and can very slowly pass within place 6 and then goes inwards more quickly and stops at 5 side. So the torsion of 6°·3 is close upon right. 6°·2 is not enough, for then the vessel goes outwards from 6 towards a higher place. Take 6°·3 as right at 1 o'clk., which corrected is 0°·3 R, so that the object is a trace on the paramagnetic side of Zero.

12978. *Hydrogen*, XIII, in bath at 0°, for bath still seems good–went in at 1^h 10′ and out at . Torsion 15°, too little–20°–the currents formed in the air within the cell as the vessel is cooling move it about more or less for 10′, 15′ or 20′–20°·5 too much torsion–20°·3–20°–19°·8–is quiet in the cell now, having been

in 20 minutes—19°·5—19°·4 or 19°·3—19°·4 at 1^{h} 36′; but then went on to lower numbers after having been quiescent—as 19°·2—19°—18°·6—18°·4—and was 18°·3 at 1^{h} 51′. Still, these last effects did not quite satisfy me, and I am not quite sure whether they are due to low temperature only or to currents in the box produced by a final process of elevation of temperature—for when the object was taken out, it did not seem so cold—it did not condense the moisture of the air into frost so fast as when *oxygen* was employed (12976, 7). The bath by a thermometer was at 3° F without stirring—but the cell is on one side of the bath.

12979. So poured off the fluid of the bath—stirred up its contents of solid salt, ice and solution, and filled up with a new mixture; is now good again. Put in the hydrogen at 2^{h} 10′. The last torsion of 18°·3 was too much—but the hydrogen had to cool—made the torsion 15°—16°·5—17°·5—18° at 2^{h} 18′, and then left it whilst I dined. At 2^{h} 45′—18° was too much. 17°·5—17°·2—both good evidently—17°—16°·6—16°·7—or 16°·75. This last corrected is **10°·15** for cold hydrogen.

12980. Poured off liquor of old bath and stirred up the contents and added fresh salt and ice. *Nitrogen* in XIII, put in to the cell at 3^{h} 6′. Torsion 16°·7—15°—10°—12°—14°—16°—17°—all this variation the effects of currents in the cell—17°·5. At 3^{h} 28′ the torsion steadily 17°·5 and the currents apparently over. Stirred up the corners of the bath by the cell, removing the salt which had settled there—after the beam had settled, the torsion was 17°·5 and then rose to 18° and 18°·5. The corner stirring is good. At 3^{h} 40′ the torsion was 18°·5, which being corrected gives **11°·9** as the torsion for cold nitrogen.

12981. Refreshed the cold bath (12979). Put *Air* in XIII, in at 3^{h} 53′. Torsion 14°—stirred out the angles rather closely by thin stick at 4^{h} 4′. Torsion 14°·5—14°·7—14°·8 or 14°·9 at 4^{h} 15′ or 22 minutes from the beginning. Was 14°·8 at 4^{h} 20′ when the vessel was taken out. So 14·8 corrected by 6·6 gives **8°·2** *for Air cold.*

12982. Refreshed the cold bath, clearing out the salt from behind the cell, which is quite necessary, and put in *Carbonic Acid gas* in XIII; into cell at 4^{h} 30′. The torsion was 14°·8—17°—18°—19°—19°·5 at 4^{h} 42′ and the vessel becoming steady—19°·3—stirred up the angles, all well and clear—19°—19°·3 at 4^{h} 53′—

19°·5–19°·4 at 4^h 57′. The vessel was well cooled. So 19·4 corrected by 6·6 gives **12°·8** as the torsion for Carbonic acid gas.

12983. *Steam vacuum.* A steam vacuum was made in XIII, only a small drop of water remaining in the lower neck. This was put into an excellent and cleared cold bath at 5^h 12′. By 5^h 25′ it was very steady and apparently well cooled. The torsion then was 19°·2–19°–18°·5–18°·4 at 5^h 30′. Now 18°·4 corrected by 6°·6 becomes **11°·8** for this vacuum.

12984. Then the vessel was set aside–the cold bath removed–the box doors set open for half an hour–the glass cell introduced (12966), also a thermometer which shewed the box temperature; and the steam vacuum XIII (12983) was hung on the beam in the cell. The torsion was 19°·4–20°–20°·5–21°, the temperature being 62° F. This corrected gives **14°·4** as the torsion of vacuum in warm air. When the vacuum was opened under water, it proved to be very good–better than the air pump vacuum (12968, 71).

12985. Then Air again in XIII, at temp. of 62° F. 19°–18°·5–18°·3, which corrected gives **11°·7** for warm air.

12986. The Zero was now again examined with balance as at the beginning and found to be still 6°·6D as at the first.

12987. The vessel XIII was then filled with distilled water and weighed 207 grains–and then being put on the beam in the air cell the torsion appd. to be 100°–92°–93°–92°·4, which corrected is **85°·8** for water.

12988. The slow and few vibrations in the air cell at common temperatures are very distinct and very useful, and are due I think to the obstructed external air, i.e. the air between the object and the vessel. But is it possible that the vessel may have any action as surrounding glass? And would a cell of copper, being a good conductor, shew any difference? Well to ascertain this hereafter.

12989. These results tabulated stand as follows (12951):

	at 61° F.	0° F.	
Oxygen	1°·8	0·3 R	2·1[1]
Air	11·7	8°·2	3·5 or 3
	11·2		
Vacuum	14·4	11°·8	2·6 or 1·9
	13·7		
Hydrogen	14°·3	10°·15	4·15
Nitrogen	14°·3	11°·9	2·4
Carb. Acid Gas	14°·4	12°·8	1·6
Water	85°·8		

[1] This column is in pencil.

12990. In preparation for experiments at low temperature, I have constructed a cardboard and mica screen tube about the beam, which encloses all but the object extremity, and seems as if it would intercept air currents within the box due to the presence of the cold bath.

12991. Have provided the cold bath with an outer double jacket of cotton wadding dry, cut away at the part between the bath and the magnetic poles. This was found in the course of the day to have a very excellent effect in keeping the bath in condition and diminishing the internal currents of air.

12992. I have arranged a piece of card board horizontally over the chief part of the bath, supporting it by the wadding, so that by its projection 2 inches or more over the bath, should restrain the currents chiefly to the part beneath its level.

12993. When gas had been on a couple of hours and the irons were away, and the beam balanced with 2 weights, all being at the common temperature of 60° F., the Zero of index was at 13° or 13°·05 D. The vessel employed to-day is No. XIII (12930, 69)—the place 6—the distance 0·9 as before.

12994. No. XIII, *Air*, at 60° F. 25°·6—25°—24°·4, this corrected by 13° gives 11°·4 for Air, which is the mean of the experiments on the 11th (12989); so that the results are for the same force, distance, etc. and ought to work in with those in correction of them.

12995. *Hydrogen* in No. XIII, at 60°·2 F.—a little difficulty in the sealing because of the lead reduced by the hydrogen and heat—requires attention to be sure. When in the cell, I put the index at 27°·3, equal to 14°·3 which was the former last result (12974, 89), and without any further change the beam settled to the place 6. This gives one assurance in the results, since it is a repetition of the former result. So 27°·3, which is the present place of this hydrogen, corrected by 13°, gives 14°·3 as the torsion for the warm hydrogen and vessel.

12996. Now the cold bath: fresh and good and at 0°. The *hydrogen* into the cell—was plenty of vibrating room both on inside and outside of place 6. Evidence of currents in the cell at first as the object cools. The wadding good; it rises $\frac{2}{3}$ of an inch above the edge of the bath, which has of course the flannel and

the paper jackets on besides ( )—the horizontal pastboard shade also good. The hydrogen goes in at first (the former torsion remaining) but the object in this and the other cases comes out again as the cooling is more perfect. The hydrogen was put in at $10^h\ 35'$—20 minutes afterwards, it was within place 6, with the old torsion. Made torsion $26^\circ{\cdot}5$. On taking off the horizontal screen from the bath and putting up an end screen about object end of beam, still the place seemed the same, so I hope that there are no distributing currents in the box; but notwithstanding, I restored the horizontal screen and took away the end screen, and intend to have all the experiments alike in this respect. Torsion successively 26°—$25^\circ{\cdot}5$—25° at $11^h\ 5'$—$24^\circ{\cdot}6$ at $11^h\ 10'$. The corners of the bath stirred and cleared out as to salt—settled at place 5·95 at $11^h\ 20'$, or 45 minutes from the first introduction, with the same torsion as that of $11^h\ 10'$, i.e. $24^\circ{\cdot}6$. This amount corrected gives $11^\circ{\cdot}6$ as the expression for hydrogen.

12997. *Nitrogen* in XIII, and cold bath renewed and excellent. The Nitrogen reddened a little by N. Gas. The vessel was in cold cell at $11^h\ 40'$—plenty of room to vibrate. The torsion is $24^\circ{\cdot}6$ as left from the last expt. At $11^h\ 50'$ or in 10 minutes, made torsion 24°, which was then very close upon right, place being 5·92. In 10 minutes more the place was exactly 6—in 5 minutes more it was the same or rather a little out, 6·02. So the torsion now after 25 minutes is 24°. Stirred up the solution and cleared the corners, and then left the beam to settle—by $12^h\ 15'$ the place was 5·97. So accept 24° as the torsion for Nitrogen after 35 minutes in the cold cell. When taken out, the object instantly froze the condensed dew, and was evidently very cold. Bath at 0°, and also in the Hydrogen case. So 24° corrected by 13° gives 11° as the torsion for *Nitrogen*.

12998. *Carbonic acid gas*. Bath good. The vessel into the cell at $12^h\ 35'$—plenty of vibrating room. At $12^h\ 50'$, the object having gone in at first, was now out at place 6·2, with the old 24° of torsion—$24^\circ{\cdot}2$—$24^\circ{\cdot}4$—$24^\circ{\cdot}5$. At 1 o'clk. stirred the angles and then, being left till $1^h\ 5'$, was at place 6. It then went out steadily—$24^\circ{\cdot}6$—25° at $1^h\ 12'$—$25^\circ{\cdot}2$—$25^\circ{\cdot}3$ at $1^h\ 16'$ and steady. It has been in 41 minutes and I believe $25^\circ{\cdot}3$ to be the torsion, which corrected by 13 gives $12^\circ{\cdot}3$ as the true expression.

12999. *Oxygen.* Bath good, is remade each time. Vessel in at 1^h 35′—and the stops up—0·3 on each side of place 6. Putting the torsion at Zero or 13° D, the oxygen goes in or up to magnet at 1^h 52′, i.e. after 17 minutes. Made the torsion 12°. At 2 o'clk. with 12°—the beam goes from place 6·1 up to 5·7, so 12° is too much. Made it 11°·5. At 2^h 13′—the beam goes out from place 5·9 to 6·3, so 11°·5 is a trace too little. Stirred up the bath and corners at 2^h 20′. At 2^h 30′—the torsion is very nearly right—the beam seems almost at rest, but after going very slowly from 5·8 out to 5·9, it returned as slowly to and past 5·8. So 11°·5 is not too little; is perhaps as near as can be. A street vibration now sent it out to place 6—there it hung, and at 6·05—6·01—as if still—left it there at 2^h 30′ or 55 minutes from the beginning. At 3^h 10′ found it at 5·8—but the mere motion about the balance made it go out to 5·9 and gradually to 6. It is very nearly equipoised at this torsion of 11°·4. The constancy looks well, but I must see if there is any correction for box current (13002). At present the torsion of cold oxygen is 1°·6 reverse torsion.

13000. *Air.* Bath good. In at 3^h 30′. Put the torsion index to 24°. This at 3^h 45′ or in 15 minutes was too much—22°—21°—21°·5—22°—22°·5—22°·1 at 4 o'clk. or in 30 minutes. Stirred the corners of the bath and left it. At 4^h 10′ the torsion was 21°·7, i.e. in 40 minutes from the beginning. Accept this expression, which corrected by 13° gives 8°·7 for Air.

13001. *Steam Vacuum* (found to be excellent (12984)). Into a fresh cold bath at 0° at 4^h 43′—the torsion then 24°·5—was 24°·8 at 4^h 54′—25°—24°·9. At 5^h 5′ stirred the bath well and left it. At 5^h 10′ the beam steady and at place 5·95. At 5^h 15′ or 32 minutes from the first immersion, the place still the same. So the torsion for the Vacuum is 24°·9, or corrected 11°·9 at temperature of 0°. The object was very cold, as all the objects were.

13002. Removed the object, balanced the beam light, left the cold bath in, and all in usual form; placed the torsion index at 13° and left all to see what deflection the currents in the box acting on the partly sheltered beam might do. This was at 5^h 18′. In 20 minutes after, the extreme places of the beam were 5·8—7—6·1—6·8—6·6—6·9. The beam tended to keep its place about

these last numbers, and did not vibrate regularly but as if subject to slight feeble currents, and 6·75 is the mean place. So current throws the beam outwards a little as might be expected (   ).

13003. The cold bath was taken out and the box thrown open above and below at $5^h\ 45'$ to remove cold air and low temperature, but because of the wadding very little lowng. effect in the temperature had occurred. After half an hour the temperature in the box was 62° F., and then the Vacuum was again put on the beam and its place and torsion ascertd.

13004. *Vacuum* XIII *at 62° F.*, distance 0·9. Torsion 25°. In glass cell stood at place 7 with 25°–fearing electricity, I replaced the glass cell by a copper cell and brushed the object with the damp brush. Then in copper cell still with 25° place was 7–torsion raised to 26°–27°–27°·5–27°·8. Then I moved the copper cell so that at one time the object should be near it on the outer side and at another near it on the inner side, expecting that any electric effect would here be manifested–but the torsion required for place 6 was still in all the cases 27°·8. This corrected gives 14°·8 as the torsion due to a vacuum. The vacuum was now opened under water and found to be excellent.

13005. *Air* again in XIII, but vessel damp inside. Temperature 62° F. Torsion 24°·6–24°·7–25°. This corrected gives 12° torsion for air.

13006. The beam now balanced with two weights and its Zero ascertd. It proved to be not 13° but 12°·7, and so a correction to make. Now this correction has to be added, i.e. the torsion is greater by the 0·3. But the correction for cold current (13002), which being 0·75 of place is very nearly of the same amount or 0·25, is for these cold experiments subtractive, i.e. the torsion had to overcome this effect as well as the magnetic effect, and therefore so much must be taken off the magnetic account and put to the current account. So 0·25 may be taken from the cold results. As to the change of torsion, it may have been graduall from the first cold results; and if I assume it so, then the sums to be added may be successively 0·03–0·06–0·1–0·13–0·16–0·2–0·25–0·3; and if so, the following page shews the results[1].

[1] I.e. par. 13007.

13007.

	F.				
Air at	60°	11°·4	. . . .	.	11°·4
Hydrogen	62°	14°·3	. . . .	.	14°·3
Hydrogen	0°	11°·6	−0·25+0·03	. .	11°·38
Nitrogen	0°	11°	−0·25+0·06	. .	10°·81
C. A. Gas	0°	12°·3	−0·25+0·1	. .	12°·15
Oxygen	0°	1°·6 R	+0·25−0·13	. .	1°·72 R
Air	0°	8°·7	−0·25+0·16	. .	8°·61
Vacuum	0°	11°·9	−0·25+0·2	. .	11°·85
Vacuum	62°	14°·8	+0·25	. .	15°·05
Air	62°	12°	+0·3	. .	12°·3

D—direct torsion
R—reverse torsion

13008. Or again thus:

	60° F.		0° F.	mean at 60° or 62° F.	about 60° F.	*differences*
Oxygen	1°·8 *D*[1]		1°·72 R		1·8 D	3·52
Air	11°·4 12·3	11·7 11·2	8·61		11°·65	3·05
Vacuum	15°·05	14·4 13·7	11°·85		14°·38	2·43
Hydrogen	14°·3	14·3	11°·38		14°·3	2·92
Nitrogen	14	14·3	10°·81		14°·15	3·34
Carb. A. Gas	14·4		12°·15		14°·4	2·25
Water	85·8				85°·8	

13009. I incline to trust the cold expts. of to-day, having taken all care with the beam and bath—but not the cold results of former days. I incline to trust the warm results of to-day and of the last time made with the same vessel and distance ( ) but not those before. The mean results are above.

See these results considered on the 15 Feby. 1853 ( ).

[1] Numbers in smallest type are in pencil in the MS.

14 DECR. 1852.

13011[1]. Have been making a few general expts. on Stokes' phenomenon in reference to convenient sources of light for experiments at the magnet. Used the Solution of Sulphate of Quinine as the dispersing medium generally, either in a flat glass dish or in a glass cylinder.

13012. *Hydrogen.* Its flame good. The oxyhydrogen flame very fine—far stronger for the same amount of hydrogen burned. Now makes glass luminous and gives a very good expt. for an audience if burnt horizontally over a large dish of Sulphate of Quinine solution—or upright in a thick glass jar. The light of an explosion in a glass Cavendish vessel is chiefly due to this effect. The small oxyhydrogen jet will do well for the magnet. Hydrogen jet flame in bottle of oxygen—good; in a bottle of chlorine—bad—no sensible effect whatever.

13013. *Cyanogen flame*—very fine effect—over dish and by the side of the glass.

13014. *Carbonic oxide*—ordinary flame—very fine effects; the rays are low in character and abundant and penetrate the solution to much depth. Must try an Oxy.-Carb. Oxide flame—it proves to be very good.

13015. *Sulphur in Oxygen*—very fine effect—great abundance of low rays.

13016. *Weak Alcohol* and *Strong Alcohol* flame—not much difference for the same amount of alcohol. *Ether* flame—pretty good effect.

13017. *Oxy. alcohol* jet. Much better than the flame without oxygen.

13018. *Oxyether* jet. Not so much improvement as in the Alcohol.

13019. *Oxycoalgas* jet flame. So so. Coal gas made to burn pale by mixture of ammonia produces much better effect than the ordinary coal gas—and then with oxygen, is very good effect.

13020. *Iron burning*—not easily sensible. Burning Zinc—the same.

[1] 13010 is omitted in the manuscript.

13021. Flame of *hot glass rod in Ether vapour and Air*—no sensible effect.

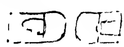
13022. Platina wire ignited in hydrogen—no apparent effect.

13023. Pale roaring gas flame—nothing particular.

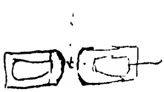
18 DECR. 1852.

13024. Arranged the double cone at the Great Electro-magnet with a flint glass prism 4 feet distant and a lamp flame 4 feet from that, so as to throw the spectrum across the magnetic field and by a little adjustment bring any of the rays in the spectrum across the most intense part of the magnetic field. But whichever ray, from the extreme violet to the red, was taken, no effect on it was perceived when the Electro-magnet was either excited, or thrown out of action, or the polarities quickly reversed. The different rays were admitted through a slit placed about 3 inches from the prism, but with the same negative result.

13025. The hollow poles were used at the electromagnet, both with air and also with a cube of heavy glass in the magnetic field, but the result was negative as to action on a ray of light.



13026. A little silvered glass reflector was put in the middle of the magnetic field, so as to send the ray out axially, so as to remove any antagonistic effect on the opposite sides of the magnetic axis. The result was negative, both when air was in the magnetic field and also when heavy glass was between the mirror and the prism and close to the former.



13027. The heavy glass was in the center of the magnetic field—a spirit lamp on the one side and a tube of solution of Sul. quinia on the other. Making or unmaking the magnet produced no sensible effect on those rays made visible by the sol. quinia.

[13028][1].

[1] Par. 13028 has been struck out and rewritten as par. 13052. See p. 255.

15 FEBY. 1853.

13029. At last I am able to resume for a little while the consideration of the results of the 16th Novr. last year (13007, 8, etc.). It would appear from 13008 that all the bodies on the balance are *more diamagnetic as they are hotter.*

13030. Now is this effect due to the mutual relation of the *glass vessel* and the *surrounding air* on the cell: for they are the only two things common to all the experiments, and where air was *inside* as well as *outside* (13000), it would appear to be only a question of air and glass, i.e. if all effects of disturbance in the box generally were fully guarded against. If such were the case, then *glass* would become more diamagnetic by heat or more paramagnetic by cold than *Air*: and of course *Air* less diamagnetic by heat and less paramagnetic by cold than *glass*. As both are cooled together, they approximate in character; as they are heated they separate, the difference in the present case as presented by one body over the other being about 3° of torsion in a change from 0° to 60° F. Both may have changed much more than appears, but a change leaving this difference has occurred.

13031. This might be, and still the constituents of common air not change alike. But in that case, if the two constituents of air were separately employed to fill the bulb, experiments of a like kind would then shew which of them, the Oxygen or the Nitrogen, changed by cold either with or against the glass. Now when Nitrogen was in the bulb, the difference of torsion for 0° and 60° F. is 3°·34, or more than before but still in the same direction—as if Nitrogen as well as glass was more diamagnetic than air as the temperature rose, or more paramagnetic than it as the temperature fell. Such a result would indicate that as the temperature fell, Nitrogen increased in paramagnetic character more rapidly than oxygen, which can hardly be unless Nitrogen becomes paramagnetic by *cold*, and this is not likely at 0° F.

13032. Again, take *oxygen*, or the other element of *air*, in the vessel. Now the difference between warm and cold (13008) is greater than in any other case, being 3°·52 of torsion—though not much greater. Still, it tends to shew that *oxygen* is more

paramagnetic by cold than air, and consequently than nitrogen; but this result and the last with nitrogen cannot both be true. In fact the difference is greater with Oxygen alone or with Nitrogen alone than with the mixture of Oxygen and Nitrogen or air. One would expect that with true results, Air ought to be somewhere *between* Oxygen and Nitrogen and much nearer to Nitrogen than to Oxygen.

13033. I suspect the final effect does not depend upon the contents of the vessel but on something external to it—as currents, etc. And this view is supported by the fact that Hydrogen when in the vessel is nearly up to Air, the difference between it at 0° and 60° being 2°·92 of torsion.

13034. A vacuum shews the difference of 2°·43 (13008) and Carbonic acid gas that of 2°·25 of torsion. These are the smallest differences, but still, they are in the same direction as the others; and are equal to $\frac{2}{3}$rds of the largest, so as to indicate rather errors of experiment than any effect of real physical difference amongst them and the rest.

13035. Again, looking at Nitrogen, Air and Oxygen. The difference between Nitrogen and Oxygen at 60° is 12°·35 of torsion—that between Nitrogen and Air at 60° is 2°·5, which is nearly $\frac{1}{5}$ of the former, as it ought to be. Also the difference between Nitrogen and Air at 0° F. is 2°·2 of torsion, and that between Nitrogen and Oxygen at 0° F. is 12°·53, or as nearly as may be the same as when warm. So that the relation of Nitrogen, Air and Oxygen does not seem to be changed in descending from 60° to 0° F.

13036. The difference between a Vacuum and Oxygen at 60° F. is 12°·58 torsion, and the difference between them at 0° F. is 13°·57 torsion, only 1° in favour of oxygen, which is so small that it does not look like an increase of the paramagnetic power of oxygen by cold, but rather a difference due to inevitable uncertainty in the experiment.

13037. So also the difference between Carbonic acid and oxygen is 12°·6 of torsion at 60° F. and 13°·87 torsion at 0° F.—too little to shew much effect of cold on oxygen beyond any effect on carbonic acid—and probably a result of experimental error.

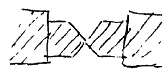
13038. It would seem therefore that change of temperature

between 60° F. and 0° F. does not produce any effect on oxygen other than that it produces on Nitrogen and probably other gases tried or even a vacuum.

16 APRIL 1853.

13039. The affection of the spark produced on breaking contact of a Voltaic battery near the poles of a great horseshoe magnet. See De la Rive, 1846, 7. Phil. Trans. 1846, 7. Page also, Silliman's Journal, 1850, vol. x, p. 349–vol. xi, 86, 191. Pouillet–his Elements, 1853, vol. i, p. 329.

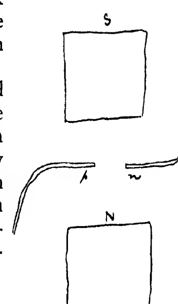
13040. Our great Electro magnet excited by 20 pr. of Grove's plates, and the copper wires continued so that contact was completed or broken in the magnetic field. When broken, of course the spark due to secondary induction was very powerful. The terminals were put up with the double cone, and the wire ends joined in the neighbourhood of the cone and broken again. As all the effects to be noticed occur only at the breaking contact, that may be understood in the following description.



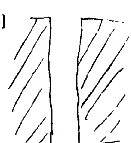
13041. On breaking contact of the copper terminals at a distance, there is a bright momentary discharge accompanied with a snapping noise, i.e. one sound or snap. Now this snap was much louder in the close vicinity of the magnetic axis than at a distance. Also in the former case the light of the spark was larger, though I think not brighter, and it had a flame, diffused and red, associated with it, sometimes on one side and sometimes on the other.

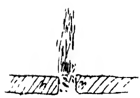
13042*. When two flat faced poles were opposed and the spark taken between them, the effect was better than round the double cone, and there appeared to be more regularity in the position of the flame parts of the spark.

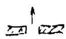
13043. When the terminals were dismissed and the spark caused about the large faces of the poles, then the effects (peculiar) were equally strong and the law of action came out clearly. The diagram shews the faces of the two poles of the Electro magnet as they stood up through the table. The one marked S attracted the North end of a magnetic needle and the one marked N attracted the south end of the needle; the ends of the wires *p* and *n* came up as depicted–the end *p* was the positive electrode–the end *n* the negative electrode.

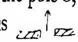
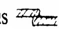
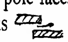
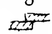
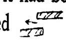


* [13042]

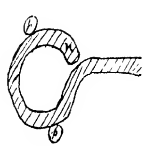




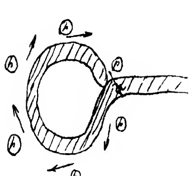
13044. Now when the ends p and n were brought together and then separated, away from the magnet, there was the usual bright secondary spark and the snap noise. When they were so dealt with close over the face of either pole, there was the spark, but associated with it on one side a reddish flame extending sometimes almost an inch, and when the bright part was shaded from the eye it was seen to extend a full inch. The direction of this jet of flame has now to be observed and shall be indicated by an arrow  thus.

13045. Operating over the middle of the pole S, when the wires were thus  and separated thus , the jet was as indicated, proceeding horizontally and parallel to the surface of the pole face. When the ends, being thus  were separated thus , then the jet of flame was still horizontal but its direction at right angles to that it had before. When the ends, being thus  were separated  thus, then the flame, still horizontal, went to the left hand 180° round, or 90° from the first position on the contrary side.

13046. All the sparks then taken were horizontal or parallel to the face of the pole. When the wires were so separated that a vertical spark was taken, i.e. perpendicular to the face of the pole and therefore in the direction of the *lines of force*, then there was *no* projected flame. When the separation was such that the spark was inclined to the face of the pole, there was flame and that increased until the spark was parallel to the face of the pole.



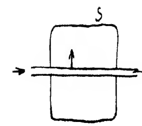
13047. When the n terminal was bent into a ring and laid on the middle of the face of the pole S, the p terminal could be placed any where against its side, as at p or p; and then being separated by a horizontal motion, the sparks could be obtained at any place all round the ring and therefore in every direction in a horizontal plane. The consequence was that the flame was always horizontal and at right angles with the direction of the spark and always on one side in relation to its direction; the figure will indicate this direction.



13048. The like effects and directions occurred over every part of the face of the pole S up to its edges—there was no difference any where.

13049. With the wire ends in the same positions over the pole N, exactly the same effects, with this difference, that the flame was in every case projected in the *opposite direction.*

13050. So this appears to be simply the case of a strong momentary current which, being produced at right angles to the magnetic lines of force, tends to and is knocked across them with such impetus as to project the flame and cause the increased noise. The law of motion of a wire carrying a current is represented thus, and it is seen that the wire would move as the flame moved–so all these effects agree. And the different directions of the darted flame round the ring end of the *n* wire (13047, 8) all depend upon the direction which the spark (and current) had at the place of its occurrence. When the flash seems to dart along the side of one wire or the other or from both, these differences do not depend upon the wires but upon the position and direction of the spark. The reason of the horizontal projection of the flame is evident and also why the spark in the direction of the lines of magnetic force is not affected. The contrary direction at the two poles is explained.



13051. The spark between charcoal or carbon terminals was affected as to the flame in the same way, but there was much less sound. I think that is due to the longer continuance of the charcoal spark, perhaps because of the more easy diffusion of carbon in the place of action, and so a gradual letting down of the tension or condition instead of the sudden one obtained when metals are used. I think it is a case of time.

13052. *Quet* has well described the effect and referred it to the true action of the Magnet on the Electric current, and I do not think I need go further into it. Comptes Rendus, 1852, xxxiv, 805.

16 MAY 1853.

13053. In regard to the possibility of the Magnetic force affecting a change in the character of rays *emanating* from an object place in the center of an intense magnetic field. The great Electromagnet was supplied with 20 pair of Grove's plates and had terminals arranged as in the figure. A small oxyhydrogen jet was adjusted in the very center of action, and then the flame examined whilst submitted to the intermit[t]ed action of the magnet.

13054. Its light examined by the prism shewed no sensible difference whether the magnetism was on or off. It shewed no difference when examined in various ways by polarizers, i.e. Nicol's prisms. It shewed no difference as regarded Sulphate of quinia in solution placed either directly beneath it or at one side of it. If the rays were examined as proceeding out equatorially or axially, still no difference was seen. No permanent difference was seen, neither any difference on making or breaking the voltaic circuit.

13055. When a jet of Carbonic oxide gas was burnt which gave rays rendered manifest by the quinia solution, i.e. Stokes' rays, still no effect of the magnetism on them could be discovered.

13056. When a small helix of platina wire ignited by a voltaic battery was employed as the source of rays, still no sensible effect of the magnetism upon the rays could be made out.

13057. *All the results were Negative.* See Notes of 18 Decr. 1852. 7808–21, 7912–22, 8683–706, 10791, 13024–27, 13053–7.

AUGUST 1853.

13058. Have begun to prepare for experimental trials on a ray of light by rotating bodies, with the hope of educing some signs of the production of an electric current, or some equivalent action, having the *ray* as its cause: hoping to evolve electricity and magnetism from light. In reference to such a research, see the former failing trials at Nos. 7808–21, 912–22, 8683–706.

13059. Look for effects both by my *thick wire* Galvanometer and thick wire helices, and also by Dubois-Reymond's *thin wire* Galvanometer and thin wire helices: using rotating bodies, as Rock crystal, oil of turpentine, syrup, etc. as the cores of the helices—and also heavy glass as the core of a helix subject to the Logeman Magnet at the time when the ray passes through it. For I must not forget the difference of the latter rotation from that of the quartz or other bodies where the rotating condition is natural.

13060*. There is a fine crystal of Silica at the British Museum, having the oblique plagiedral facets on several of the angles. Herschell says it must rotate a ray and so says Darker. The whole crystal is very clear and good, having but few flaws comparitively.

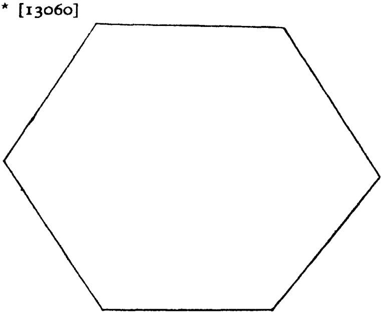
13061. Mr G. R. Waterhouse has charge now of this department and I have applied to him by letter (7 August) by his advice for the loan of the crystal out of the Museum.

13062. This is intended for the large wire helix. By putting a cup upon the pyramid end, that can then be covered with Canada balsam or Camphine and then a sun's ray made to enter parallel to the axis of the crystal.

AUG. 10, 1853.

13063. Dubois-Reymond's Galvanometer cannot be moved about and I find it difficult to procure a *Sun station* in sight of it. The best I can think of at present is the top of the *Museum roof*

* [13060]



A guage of this size [diagram has been reduced to $\frac{1}{2}$ scale] in card board freely admits the crystal at the largest part.

The whole length is $13\frac{3}{8}$ inches. About $8\frac{1}{2}$ inches may be considered as body, $2\frac{1}{4}$ terminal pyramid, very good, and $2\frac{1}{8}$ of diminishing base.

at the N.W. corner. At this time of the year the Sun is on that corner long before 9 o'clk. After a time, the shadow of the tree trunk comes partially over it—then it clears—at 2 o'clk. P.M. the shadow of our chimneys approach it and at 20′ to 3 o'clk. it is in the shade of the house and after that not available.

11 AUGUST 1853.

13064. Tried a few fluids for their rotation power over a polarized ray in a tube about 2 inches long. A solution of Tartaric acid had little or none. Sol. of Camphor in Alcohol a little. Camphine better but not much. Saturated solution of Cane sugar was the best, but the rotation here was not above 25°. By the bye, I took the analyzer round in the same direction for all these; i.e. as the hands of a watch, but I had not ascertained whether their rotations were in the same direction. So solution of sugar appears to be the best—but how exceedingly inferior to Silica; the same length of rock crystal would have revolved the ray very many times.

13065. If a crystal of Silica be cupped over the pyramid by a fluid and held upright, the sun's rays falling on to the surface of the fluid will be refracted, but not parallel to the axis of the prism. If the prism be parallel to the incident ray, still the ray striking the surface of the fluid obliquely will not remain parallel to the axis of the crystal. In a more upright or intermediate position, the ray after it has entered the fluid will be parallel to the axis of the crystal, and if the fluid have a refracting power like that of the crystal, the ray will pass through the crystal itself parallel to the axis. Now Camphine is not much below quartz in refracting power, and on filling a glass jar with that fluid and then inclining it until the light went well down the middle, and illuminated the bottom, it made an angle of 25° nearly with the perpendicular at 11 o'clk. of this day. Such a position would be a very convenient one for the prism in its helix.

13066. The following are some refractive powers that concern me in the cupping of the Rock crystal—

Rock crystal	1·548
Canada balsam	1·549
Oil turpentine	1·475
Oil Olive	1·470

Water	1·336
Syrup	
Melted Sugar	1·554

13067. Placing a rock crystal in a cylindrical glass jar and covering it over with *Syrup*, there was but little reflection of light at the meeting of the two surfaces and but little refraction. A wire looked at through both was distorted in appearance only a very little even at the angles of the crystal. When the crystal was in *camphine*, the effect was still better and the wire even less distorted. Canada balsam is too thick alone to give a level surface quickly, but I think that a mixture of Canada balsam and camphine will do.

13068. *Polarizing plates.* The right angle for a maximum effect is 56° 15′ with the perpendicular, or 33° 45′ with the incident ray.

12 AUG. 1853.

13069. Canada balsam and Camphine mix with difficulty at common temperatures, but if heated in a water bath and well agitated, they mix perfectly in any proportions; 1 vol. of each makes rather too thin a fluid, thinner than treacle; 2 and 1 make a thicker and also a more refractive media and one nearer to the Rock crystal.

13070. A paper cell put round the end of a crystal and attached by Gum holds exceedingly well; and the 2+1 mixture being put into the cell so as to cover the pyramid, it was then very easy to look into the crystal and examine every place and side within. So that this arrangement will do very well. By exposure to air the camphine evaporates and leaves a hardened surface, which when the fluid is inclined produces a corrugated skin surface—but a single drop of camphine restores all to good order again. When the cone of rays from a lens was sent in at the top surface, the focus appeared on the side of the paper almost as well whether the cone passed through the fluid alone or through the fluid and crystal.

13071. *Dubois-Reymond's Galvanometer in window.* A current which goes in at 2 and out at 4 sends the east end of the needles to the North and vice versa. The little magnet is too powerful at present, so that the needles rest on either side of their proper position.

13072. Have been engaged in preparing many new pieces of apparatus for the trial of a ray of light and any power it may have of evolving electricity when made to rotate either in Rock crystal—or liquids as solution of sugar, camphine, etc.—or in heavy glass under the Logeman magnet effect.

13073. *Galvanometers.* First there is *mine*, with thick wire for currents of low intensity (See Exp. Researches 3123).

13074. *Dubois-Reymond's new Galvanometer* belonging to Dr. Bence Jones. He has just completed and arranged it. It is in fine condition and more sensible than the first one. It contains 28,790 convolutions of silked wire, all of which are imbued with shell lac from its solution, to increase the insulating power of the silk. The length of wire is considered as being , for the whole weight is grammes and feet of it weigh grammes.

13075. *Rock crystal and fine wire helix.* A fine crystal, $4\frac{1}{8}$ inches in length and of an average diameter of $1\frac{1}{2}$ of an inch, has been selected for its clearness and fitness. It has very few flaws—has both ends cut off perpendicular to the axis and polished—is found by trial to rotate a ray (from a spirit lamp flame) well—presents above rings and consequently rotates this ray times. The direction of rotation is to an observer looking through it and therefore receiving the coming ray, with or against[1] the motion of watch hands. Mr Darker has wound round this crystal 10,100 feet of silked copper wire in a helix having a diameter of $3\frac{1}{4}$ inches. The circuit is good throughout. (100 feet of the silked wire weighs 111 grains).

13076. This Rock crystal and helix has been mounted on a board in association with a good simple lens $4\frac{1}{2}$ inches in diameter and 26 inches focus. A concave lens is at pleasure associated with the former so as, when receiving the beam of about $\frac{1}{2}$ an inch diameter, then to pass it forward in a nearly parallel direction, and thus all the light from the great lens can be sent through the axis of the crystal. The whole is mounted on a stand, consisting of a globe standing in a broad ring, and thus it can be set so that the sun's rays shall fall directly on the lens and from it pass on to the crystal, i.e. the crystal can, with the lens, etc., be adjusted until parallel to the sun's ray.

[1] Words "with or against" are in pencil in the MS.

13077. Experimenting in the Model room window between the hours of 10 and 12 o'clk. with the window up–the crystal and helix was then not above 8 feet from the Galvanometer; and being connected with it and tried by a piece of platina and zinc held between the fingers, it was found that the circuit and therefore the connexion was good. The weather was variable but the sun's ray now and then pretty good. As the sun moved, the apparatus was adjusted so that when the millboard screens were removed, the compressed ray passed directly through and along the crystal.

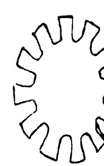
13078. When the screens were removed and the ray passed through–there was no effect at the Galvanometer. Continuing–the ray produced no effect. Shutting off the ray produced no effect. No effect whether ray continuous, or when first through or when extinguished.

13079. Introduced a bundle of *glass plates* at the right angle with the axis of the cone of rays. This would pass much less light than before, but it would be more or less *polarized.* Still no effect at the Galvanometer was produced under any of the three conditions already described–or when the ray was first polarized–or when the polarizer was taken away.

13080. *Polarized the ray* by introducing a *Nicol's prism* between the concave lens and the end of the crystal–but still no effects at the galvanometer under any variation of circumstances.

13081. Interposed a plate of *copper ruby glass* so as to transmit a fine red ray only and not all the rays. Still there was no effect at the Galvanometer, either on introducing the glass or taking it away–or under any of the former variations.

13082. I have a copper wheel cut into spaces and combined with a commutator (   ), so that as it revolves between the compressed ray and the crystal, it can rapidly alternate, either letting the ray pass or stopping it; and the commutator can be set so as to give or complete the voltaic circuit either whilst the ray is being cut off, or whilst it is let on–or in other manner. I made a brief experiment and obtained no result; but then the ray is so large that it overlapped the spaces and so different effects were taking place at the same time in different parts of the crystal. I must remove the concave lens, and introduce the wheel just at the focus, and then the conditions desired will be obtained.



13083. The Sun had now left the Model room window. Went to work therefore upstairs in the Antelecture room with My galvanometer of *thick wire* and a *large crystal* in *thick wire helix*.

13084. The crystal now employed belong[s] to Mr Dollond and is excellent in clearness and quality but not large enough in size. I have tried to obtain one from the British Museum (13060, 1) but have had much and strange impediment thrown in my way. I have persevered (see the correspondence) but if I obtain it, it will probably be at too late a period to be of any use. Mr Dollond's crystal is nearly $5\frac{1}{2}$ inches long and on an average $3\frac{1}{4}$ inches in thickness. 150 feet of copper wire 0·18 of an inch in thickness and covered with cotton has been formed into a helix upon it. The crystal has a flat and polished face nearly perpendicular to the axis, and it is not difficult to set it up parallel to the sun's ray so that it can fall directly upon it and at the same time to connect the tinned and amalgamated ends of the wire by the proper cups with my galvanometer close at hand. It is requisite however very carefully to screen off the galvanometer and the connexions from the sun's rays, otherwise thermo currents are immediately formed. These did happen, and shewed at the same time that the connexions were perfect.

13085. After this I made a few trials with sun's rays on and off. I obtained no effects—but the Sun's rays were uncertain and poor. The ruby glass and the polarizing plate were employed, but no effects obtained at the galvanometer.

13086. As to Dubois-Reymond's fine wire galvanometer, though it was affected easily by touch of finger to zinc and platina or zinc and copper terminations, still it was not good for feeble currents, and when the loose parts of the connecting wires were swung across the magnetic curves of the earth, no signs of any effect at the instrument were produced. The obstruction of such a length of wire was too much for such a feeble current as was then produced.

6 SEPTR. 1853.

13087. Rock crystal—fine wire helix—our lens—and Dubois-Reymond's Galvanometer (13074), with the revolving intercepting wheel (13082) adjusted now at the focus of the lens, the

concave lens being taken away. Now the spaces on the revolving wheel receive and include the whole of the focus, and all is right in that respect. The commutator was so adjusted that when the ray was full on to the intercepting portion of the wheel, or full on to the crystal (passing through the space cut in the edge of the wheel), then the contact spring was just passing from metal to ivory or the reverse. Hence if the ray was excluded and then the wheel revolved in one direction until it was full on to the crystal, the spring passing over the metal would make contact whilst the ray thus came on, and would consequently break contact, being on the ivory, whilst the ray was being cut off. Or if the wheel were turned the other way, contact would be broken whilst the ray came on, and made whilst the ray was cut off. And thus an accumulation of any effect due to the passing of the ray or to the stopping of the ray might be obtained.

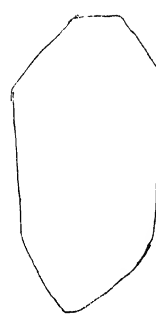
13088. The circuit was perfect when the spring was on the metal of the commutator and in that respect all was right. But whether the wheel was revolved *one way* or *the other*, no effect was produced at the galvanometer.

13089. The ray was polarized by the interposed plates (13079) but still the wheel action did nothing.

13090. The red glass was interposed and the ray used natural or polarized, but still the wheel action did nothing.

13091. When the crystal helix and commutator were in the circuit, a piece of platina and of zinc held in the fingers properly shewed a current through the galvanometer, but very much less than the effect when the crystal helix was not included; including the commutator only did not diminish the effect. Hence it appears that it opposes obstruction to the current; but from variations in the trial, it appeared also to discharge across its coils much of the electricity and so prevent it passing on to the Galvanometer. The wire of the helix is silked but it is not resinized or varnished as that of the Galvanometer is.

Proceeded to work up stairs with the large crystal and wire and my galvanometer (13073). The form of the face at which the light enters is as in the figure. The crystal was tilted and received the direct rays of the sun as before, and all the connexions were good. Began to work about ½ p. 12 o'clk. with a very good sun—the day altogether has been good.



13092. The sun's ray into the crystal permanently, produced *no effect*.

13093. The interposition of the Ruby glass, so as to produce a red ray, produced *no effect*.

13094. The bright sun's ray, all the rays intermitted with intermissions at the Galvanometer made by hand (13087), but no effect was produced.

13095. The large lens borrowed from Dr Miller has a diameter of 9 inches and a focus of 20 inches. The ray collected by this lens was sent into the crystal, but did nothing. Alternations of the ray on and off, did nothing (13082, 7). The cone was passed through a bundle of polarizing glass plates, so that many of the rays would be polarized, but still no effect was produced.

13096. The simple ray had been somewhat polarized, but no effect was so produced.

13097. The Sun's ray was then received on a bundle of glass plates at the polarizing angle, and the crystal altered in position until the reflected and polarized ray entered it parallel to the axis. But there was no effect at the Galvanometer.

13098. A glass tube, $1\frac{1}{2}$ inches in diameter and 30 inches long, terminated and closed by flat glass plates, was then filled with syrup (which rotates better than my oil turpentine) and placed in the thick wire helix (wire 0·19 of inch in diameter) connected with the galvanometer, so as to be in a horizontal position. The sun's rays were reflected horizontally by a large glass mirror silvered, and sent along the tube. There was no effect at the Galvanometer under any condition of the ray. The ray was polarized: still no effect. The ruby glass was employed: no effect. The great 9 inch lens was used and its focus directed into the end: still no effect.

13099. So that all attemps with rotating rock crystal or solution of sugar, using Sun's ray, polarized plane, etc., *failed* in evolving any signs of electricity.

14 SEPTR. 1853.

13100. This is the first fit sunny day since the 6th. Now though rotating bodies, as Rock crystal and solution of Sugar, seem to be without power over a ray as to any evolution of Electricity,

yet as their rotation is not due to a state of constraint like the rotation of heavy glass at the rotation, and in fact is not quite the same in character as the latter, so it is not yet decided that a body rotating a ray under the influence of a magnet or a current of electricity will not also, when under the same influence, produce a current when a ray is passed through it. So a square prism of heavy glass, polished at the ends, 7 inches long and 0·7 of an inch in thickness, had a helix of thick wire constructed for it, which helix would go within the poles of the Logeman magnet (12363) and allow the ends of the heavy glass to rest against the extremities of the poles*. The helix contained 55 feet of wire 0·18 of an inch in diameter—it was inches long and inches in external diameter—and was connected properly by mercury cups with my thick wire galvanometer (13073) placed about 7 feet from the magnet. The connexion was good; the warmth of the finger applied to a wire end near the mercury cup gave a ready current and action at the Galvanometer.

13101. A common glass mirror revolving on a polar axis was adjusted so as to reflect the sun's ray in a horizontal position parallel to and along the axis of the Glass prism. The use of a screen of mill board permitted this ray to pass through the glass or to be cut off at pleasure. But whether the ray was *on* or *off* made no difference at the galvanometer—no effect was produced.

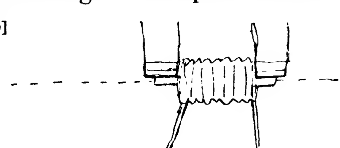
13102. The $4\frac{1}{2}$ inch lens was used (13076), its focus being either at the end of the heavy glass or an inch or two within; and with this arrangement the ray was *on* or *off*, but no galvanometer effect was produced.

13103. The transmitting polarizing glass plates were introduced into the course of the ray—but without producing any variation (13079).

13104. Alternations were made (13082), i.e. galvanometer communications were complete whilst the ray was let on but interrupted when the ray was cut off, the alternation being as rapid as the hand could make them—and many such would occur in the time of one vibration of the needle—but still no effect (as of accumulation) could be observed.

13105. So the heavy glass—with Logeman's magnet—thick wire—and my galvanometer—gives no hopes of a result in this direction.

* [13100]

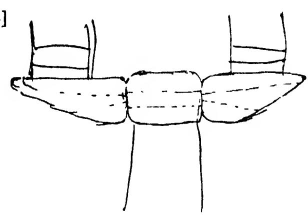


13106*. Another piece of heavy glass, $\frac{9}{8}$ inches in breadth, $\frac{5}{8}$ of an inch in thickness and $2\frac{3}{8}$ inches long, has had 1300 feet of fine wire wrapped round it to form a helix. This was adjusted at the Logeman magnet between two iron terminals which were pierced by conical holes ( ), so that a ray of light, simple or from the lens, could be passed through them and the glass in the helix. The helix was then connected by long and comparitively thick wires with Dubois-Reymond's Galvanometer below (13074, 86) and the communication found to be complete. By conducting rods of deal, a set of acoustic signals were established, and thus communications made from the Galvanometer to the helix.

13107. When the Sun's reflected ray (13101) was simply sent through the heavy glass, there was no sign of action at the galvanometer whether the ray were on or off.

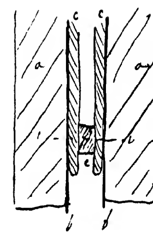
13108. When the lens was employed (13076), there was no action whether the Sun's ray were on or off. When the rays were polarized by the bundle of glass plates, there was still no effect produced. No action in any way could be obtained. So that neither by these nor by the former trials (8683–771, 92) have any evidence of the evolution of electricity by light been obtained.

* [13106]



25 FEBY. 1854.

Crushing of crystals and other bodies.



13109. The thick wire Galvanometer (11706–11) was put in place–two of the thick wire conductors connected with its wooden cups reached to a vice–the conductors were 4·25 feet and 3·66 feet long. The chops of the vice were guarded by a thick card board–the ends of the copper conductors were beaten flat and then well cleaned–a piece of bismuth, being part of a prism cut out of a crystallized mass (13115), was placed between the flat faces of the copper conductors in the chops of the vice–*a*, *a* are the vice chops–*b*, *b* the card board–*c*, *c* the copper conductors and *e* the piece of bismuth. The bismuth was a cube nearly, and so placed that the magnecrystallic axis was parallel to the line of pressure, *i*, *i*.

13110. When first put into the vice and with little pressure, the galvanometer needle was deflected, the S. end to E. about 14° and continued there–when the end of the conductor was taken out of the cup, the effect ceased, but when the circuit was complete it was renewed. Reducing the pinch at the vice made the deflection less–restoring the first slight pinch at the vice made the deflection rise up again. Increased the pinch still more and the deflection gradually diminished–still increased the pinch, and the deflection sank to Zero, then rose up on the other side and gaining 13° E., continued there very steadily for some time, half an hour or more, and then gradually fell to 2° W.

13111. The Electricity thus evolved is probably a thermo current. The finger put for a moment on either side against the copper conductor made a deflection of 20° or more, this or that way according to the side of the bismuth; but these currents fell in less than a minute when the finger off. Putting the hand merely on this or that side of the iron vice instantly caused a deflection of 32° or more, even though the heat had to travel through the mass of iron and across the pastboard. Must take care of such influences, for they are very sensitive. By touching this or that side of the vice, it was easy to bring the needle to Zero.

13112. The effect of the pressure shewed that the cause of the current produced was at the vice. Then what is the cause of

change in its direction? I think it is only this: that the vice and the copper wire conductors were at two different temperatures from handling, etc., that from the spring of the conductors there was contact more perfect with the vice at one side than the other, and hence the change of temperature would be on that side first; that by more force the conductors were brought into more perfect contact with the vice, so that the interchange of temperature could occur as well or better on the other side, and that the temperatures were such as then to cause a current in the contrary direction.

13113. I do not think that any part of the current was due to a disturbance of the condition of the bismuth by the mere act of compression. The bismuth was much squeezed, and now the line which had been that of the Magnecrystallic axis ought according to Tyndall to go equatorially.

27 FEBY. 1854.

13114. The piece of compressed bismuth appears to be very dense—attempts at cleavage shew a fine granular structure and somewhat conchoidal fracture—equal in all directions. I think it is much harder than before—and certainly seems to me to be much smaller. I have marked it I. Between the poles of a horse shoe magnet it set with the length axially, and therefore that which was the line of pressure equatorially.

1ST MARCH 1854.

13115. The prism cut out of a crystallized mass (13109) before referred to was square and long, about $1\frac{1}{4}$ inches, and the chief cleavage [illegible] was parallel to its thickness, i.e. transverse to its length. Two cubical pieces were taken off by cleavage, and calling the former piece I, these may be II and III and the remaining piece IV; the last piece is still above $\frac{1}{2}$ an inch long, its breadth not being $\frac{1}{4}$ of an inch.

13116. Piece II was placed in the vice between the conductors as before, but with the plane of cleavage parallel to the line of pressure and therefore the Magcrystallic axis transverse or perpendicular to the line of pressure. Needle of Galvanometer deflected as before, though on the contrary side, but no effect

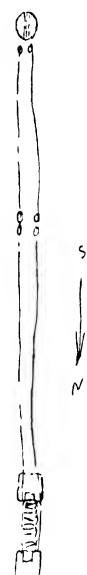
of mere pressure appeared to evolve any electricity. It appeared to me, however, that more force was required for compression than before, that in fact the bismuth was harder in this direction than in the former. The piece of bismuth cracked and split during the pressure. It was squeezed until only half its first thickness.

13117. Piece III was treated in the same way, the planes of cleavage being now perpendicular to the line of pressure, and therefore as in the first piece. I believe it gave way more easily than II. No signs of Electricity apart from the temperature. It was compressed to about half its thickness.

No. I	weighed	17·156 gr.	and its S.G.	came out as	9·58
No. II	"	45·4	"	"	9·87
No. III	"	44·1	"	"	9·33
No. IV	"	82·36	"	"	9·9

I, II and III were cracked and I believe that the cracks retained air when the bismuth was weighed in water, hence probably the reason why the S.G. seems less after pressure. The probability is that there is in that respect little or no change.

13118. No. IV pointed between Mag. poles with its length axially and its chief cleavage equatorially. Nos. I, II and III all pointed with the line of pressure equatorially. In I and III therefore the direction of pointing had been changed by the pressure.



4 MARCH 1854.

13119. *Induction of currents in fluids.* Made a fluid helix thus. An iron cylinder, 8 inches long and 1·7 inches in diameter, was selected. When put on the poles of the great horse shoe magnet, which are 6½ inches apart, it allowed of ¾ of an inch resting on each face. Being a cylinder, it did not adhere to the great magnet after it had been magnetized and demagnetized, but allowed its own state to rise and fall better than a square keeper would have done.

13120. Eight feet 4 inches of Vulcanized India rubber tube was wound into a spiral round this core. When 12 convolutions were on, 17 inches were left in two ends of 8 and 9 inches each. The seven [? twelve] convolutions contained therefore nearly 7 feet and the internal diameter of the tube was 0·25 of an inch and its external diameter about half an inch. It kept its form on the core and was

protected from pressure, so that the space within was open and clear throughout. Being measured as to its contents, the spiral part was found to contain 3 cubic inches of fluid. This helix was carefully filled with fluid so as to include no air, and then clean copper wires 0·25 of an inch in diameter were introduced at the two ends until they occupied the 8 and 9 inch ends up to the spiral, and were then tied fast so as to secure the existence of a fluid helix with copper conductors in perfect contact.

13121. The Galvanometer first used was the thick wire instrument (11706–11). It was placed 18 feet from the Magnet in the position above figured and on the same level nearly with its poles, so as to render it as little subject to the direct action of the magnet as might be. A voltaic battery of 20 pr. of Grove's plates was employed to excite the magnet. The connexions with the Galvanometer were of 0·25 of inch copper wire dipping into mercury (11708).

13122. The helix was filled with dilute S.A., 1 vol. oil Vitriol and 3 of water or Voltameter acid, which is excellent for conduction. It with the iron core was put into its place on the magnet and the connexions with the Galvanometer all prepared, but the circuit as yet broken. Then on exciting the magnet, its influence was such at this distance as to send the N. end of the needles about 5° East.

13123. I had found that the introduction of the copper wires into the fluid helix termination caused only a very slight deflection–not more than 2°.

13124. Now completed the Galvanometer circuit with the helix and again excited the magnet. There was no deflexion except that due to the distant direct magnetic action of 5°. When the battery contact was broken, there was no effect except the gradual return of the needle to its first place.

13125 Changed the direction of the Galvanometer conductors. Still there was nothing when the Magnet was excited or lowered but the direct magnetic action on the needle.

13126. Dismissed the thick wire Galvanometer and used one with a *finer wire*, having 310 convolutions and the copper wire $\frac{1}{30}$ of an inch in diameter–its length was 164 feet (50 meters). Here the needle did not stand at Zero, but about 10° or 12° either on one side or the other, because of the magnetic condition of the copper

wire. At present its set was as figured. The Galvanometer circuit was as yet not completed: then

Exciting Magnet made the N end go to right or W.

Lowering Magnet made it go to the left or E., but only a very small degree.

13127. Then completing the connexions of the Galvanometer and helix, there was now a current due to the action of the dil. S. Acid on the copper wires, which set the N end of the needles 10° or 15° more W.; it was constant, so that the needle soon came to rest. Then

exciting the Magnet made N ends of needles move E. or to the left.
Lowering the Magnet „ „ W. or to the right.

13128. Now here we have first the direct magnetic effect setting the N ends of the needles W.; then the voltaic effect of the dilute acid and copper setting the N ends more W.; and quite clear of these actions, the induced current setting the ends E. on exciting the battery and W. on lowering it. This can hardly be anything else but the induced current in the fluid. The effect was small but very constant in direction. The ends of the conducting wires were carried so nearly up to a line parallel to the axis of the helix and over it that they could not produce the effect, and I believe it is a true induction in a fluid.

13129. Uncrossed the connexions so as to set them parallel. This set the needles naturally on the other side of zero, and when the connexion broken, it stood there for the reason already given (13126). Then exciting the magnet to obtain its direct effect, the N. ends moved E. or left, and on lowering the magnet they moved W. or to the right. There was very little effect indeed of any kind and it was very slow; the needles were nearly end on to the magnet.

13130. Completed the connexions of the Galvanometer and helix; this set the N end of needles further E. as the voltaic action should do (13127). Then

exciting Magnet made N end go W. strongly in comparison,
lowering Magnet „ „ E. in like degree.

There was actually a little push or start on the needle on exciting the magnet—different to the coming on of the deflections due to direct action of the magnet or the voltaic current of Acid and copper wire. Here therefore the induced current.

13131. Breaking the Galvanometer connexions, the magnet actions were as before (13129) and the reverse of those just described. On renewing them they were again reversed, shewing the induced current.

13132. It must be remembered that the induced current is not measured merely by the reverse deflection but by counteracting the direct action of the magnets also. So that it is stronger than it appears to be and is proportionate to the sum of the two deflections.

13133. Now altered the direction of the Galvanometer connexions so as to cross them, and repeated the experiments when the needles were at rest. With no Galvanometer connexion:

Making the magnet sent N end to W. or right.
Lowering the Magnet „ „ E. or left.

On completing the connexion:

Making the Magnet sent N end to E. or left,
lowering the Magnet „ „ W. or right,

confirming the former results of induction.

13134. In order to verify the direction, I made one turn of copper wire round the core and helix in the same direction as the liquid helix, fastened its ends to the conductors, and then excited the magnet by a single pair of Grove's plates.

Exciting the magnet made the N end go to the E. or left.
Lowering the magnet „ „ W. or right.

Thus the fluid and the metal give induced currents in the *same direction.*

Repeated the Expts. with the fluid helix and whole battery:

Exciting the Magnet made N end go to E. or left.
Lowering „ „ „ W. or right.

13135. Then again, with the copper wire and 1 pair of plates as before—the effects were exactly the same as to direction. It is to be understood however that the current with the metal wire was far more powerful than that with fluid helix. The first swung the needle round, the second moved it only a few degrees.

13136. Took down the helix from the magnet—removed the conductors and the acid—and then washed out the helix. Ran a stream of water through it for some time, then filled it with distilled water and soaked it well—afterwards filled it with fresh

distilled water—cleaned the conductors well—restored them to their place in the helix—and the helix to its place on the core and at the Electro magnet. So now there is a pure water helix to be acted upon by the magnetic forces. It is of course a very bad conductor. The connecting wires were placed parallel. The near or N. end of the needle was about 20° to East and at rest. Then (having no magnet excitement) the communications were completed, but there was *no change* of the needle, proving that there was now *no chemical current*—the copper and water being insufficient to produce one.

The galvanometer connexion being continued, the battery was connected and
the Magnet excited, the N end of needle went very slowly E.
Lowered the Magnet „ „ went as slowly W.
Repeated with same results.

As this effect might include two actions, either combined or opposed, namely the direct action of the Magnet and the induced action on the hydro helix—broke the galvanometer connexions to cut off the latter—then

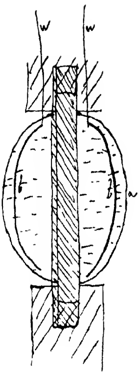
Exciting Magnet, the N end of needles went very slowly to E.
Lowering Do. Do. W.

exactly as before, both in direction, degree and time. Hence I conclude that pure water is so bad a conductor there is no sensible production of an induced current in it.

13137. With respect to the difference of Water and dil. S.A. in relation to Electrolytic conduction and their probable likeness as to conduction proper—the experiments decide nothing; for with the dilute Sulc. acid the current may exist by either electrolytic induction or conduction proper. Whatever the means of producing the current in the fluid, by induction or otherwise, still it is an axis of power and the constituent elements will tend to go out, as said in Exp. Researches, 517, 518, 524, 537, etc., being as it were expelled. But though the results do not decide the nature of the conduction, they prove that a current is induced in liquids having sufficient conducting power, and I dare say that with a much finer galvanometer wire even those in water would appear.

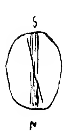
13138. Tried this *water helix* with the thick wire galvanometer,

though not with any expectation of getting signs of current ( ). Such was the case, for whether the galvanometer connexions were completed or broken, the effects at the Galvanometer remained the same. The direct magnetic action was sensible, but no signs of an induced current.



13139. Considered that if a very broad band of fluid were passed once, or half once, round the core, the induced current, though one of small intensity, would have great quantity; and that this quantity might be gathered up by metallic plates in the fluid and so carried off to the galvanometer and made sensible. Took therefore a glass dish *a*, $6\frac{1}{4}$ inches in circumference and $1\frac{1}{4}$ deep, and placed it between the magnet poles so as to be level at its edges with their upper surface. A square keeper, $12\frac{1}{2}$ inches long and 2 inches square, was placed on the poles and over the basin. Dilute Sulc. Acid (1 oil Vitriol + 3 vols. water) was poured into the basin until within $\frac{1}{4}$ of an inch of the keeper—its depth being 1 inch. Two perfectly clean platina plates, $5\frac{1}{2}$ inches long and above $1\frac{1}{4}$ wide *b*, *b*, soldered to copper wires, were put into the acid on each side of the keeper, so as to be generally parallel to it but with one edge and the copper junctions out of the fluid. In this way, any current induced in the fluid would tend to pass across from *b* to *b*, would be collected by the platina plates and might be conducted by the wires W, W, to the Galvanometer. The plates were put into the acid, the ends of their wires connected together and the whole left thus for an hour, that all voltaic tendency might settle down.

13140. After that, the fine wire Galvanometer was employed, and on completing its connexion with the plates thus, there was a little deflection produced, shewing a voltaic current and its amount; the needle stood thus. Then:



Exciting Magnet made N end of needle move to E.
Lowering Magnet " " " W.

Repeating the experiment, there was the same effect—but it was small. On interrupting the Galvanometer connexions, there was no effect on the needle on making and breaking contact. So that the effect produced when connexions were complete must have been from an *induced current.*

13141. This square keeper adhered strongly to the magnet after

the battery was disconnected and in fact sustained its state—so that the raising and lowering of the magnet power must have been much smaller than before, and hence one reason for the diminished effect. I put a sheet of paper between the magnetic poles and the keeper, but still the change in the magnet state was only slow. Still the effects were in the same direction as before.

13142. To compensate or correct this effect I employed the *round keeper* (13119). When there was *no* Galvanometer connexion, there was scarcely a trace of motion in the needle, so that in present position of the needles and strength of voltaic battery, the direct magnetic action was almost insensible. When the Galvanometer connexions were completed, there was a little action on the needle, shewing the existence of a slight voltaic action in the dish and that the connexions were good. Then

Exciting the magnet sent N end of the needle to the E.
Lowering the magnet ,, ,, ,, W.

This is the induced current—but the effect is not so good as with the helix.

13143. In order to verify the direction, etc., connected a copper wire which passed between the keeper and the dish of acid (so as to represent the latter) with the wires of the platina plates, and then used a *single pair of plates* to excite the magnet, the connexions and other things being as before.

Exciting the magnet sent N end of needle strongly to E.
Lowering the ,, ,, ,, W.

When the connexion was broken, the single pair of plates had no power to render the magnet influential by direct effect upon the needle.

13144. Here again the induced currents in the wire and in the fluid are in the same direction.

13145. The course of the connexions at the keeper as made by the helix and the dish of fluid were in contrary directions, i.e. the course with the dish is from the E. *under* the keeper to the W. But with the helix it was from the E. over the keeper round and round and at last over to the W. Now it will be seen by looking at the results that they are perfectly accordant, for the induced current with the hydro helix sent the N end to the E. when the connexions were (13127, 33, 4), and the induced

current with the dish sent the N end of the needles in the same direction when the connexions were ═══, as ought to be the case (13140, 2, 3).

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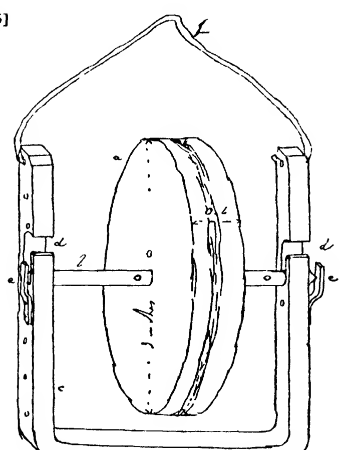
13146*. *Baden Powell's Rotation results*—his evening 3 March, 1854.

The apparatus consists of a brass disc *a*, 0·4 thick and 3 inches diameter, moving smoothly but truly on a steel axle *b* at any part—*c* is a frame made out of thick brass plate (drawing to size nearly[1]), with two notches at *d*, *d*, which let in the axle to its true bearing—the ends of the axle are cones and retained in place by the stops *e*, *e*. There are holes in the frame *c* so that by wires in these, as *f*, the frame with its axle can be hung up by a piece of braid having no torsion in any position. The braid then serves as a vertical axis about which the whole may revolve, and this axis of course passes through the center of Gravity of the disc and frame. With the addition of a bar made fast to the frame and a counterpoise, the whole may be suspended by a vertical axis more or less on one side of the disc and its particular frame—still the vertical axis and the center of Gravity of the whole is in the same line. But by using a vertical rod suspended by braid as an axis and restraining it in its place by bearing above and below, and by attaching the frame to this axis by a rod, the disc with its

[1] Reduced to $\frac{3}{4}$ scale.

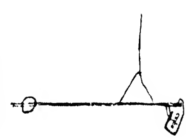
* [13146]



immediate frame can be placed at any distance from the Vertical axis, and the latter not pass through the center of Gravity of the whole. There is no real difference between this result and the last except that one half of the (equivalent) weight to be moved is taken away. The rod between the vertical axis and the disc frame may be horizontal or ascend or descend without any change in the results.

13147. There are holes made also in the steel axis and the disc; by the insertion of wires in these the disc and the axis can be fixed together—or the axis and the frame. So the disc may rotate on the axis whi[l]st the latter is fixed to the frame—or the disc may be fixed to the axis and rotate with it at the bearings of the latter in the frame—or disc, axis and frame may be all free, so that the rotation may occur either at one bearing or the other. I believe that all the peculiar effects are due to friction, momentum and the resolution of forces, and contain in reality nothing remarkable except the form of the result.

13148. The disc has a groove round its edge and by winding a thread in it and pulling it off, rapid rotation can be given. Or by introducing the bend of a double thread in the axis when it is fixed to the disc, [illegible] it out and then pulling it out as a double, very rapid and steady rotation can be given to it. The disc weighs 5766 grains and in most cases turning it by the hand is sufficient. The axle alone weighs 154 grains.



13149. In the first place the disc and frame were on a horizontal lever fixed to the frame and about 6 inches from the vertical axis. The disc was inclined to the vertical axis but the disc axis was in the same plane as the vertical axis. Then the disc was rotated, and the whole being placed in a state of rest, except the rotation, it was left to itself and quickly began to move, for the disc and frame soon revolved round the vertical axis in such a manner that its direction was the same generally as the upper parts of the moving disc; thus if the upper parts of the disc came towards the observer, then the disc and frame advanced towards the observer—if the upper part in revolving went from the observer, the disc and frame altogether went from the observer. The effect was the same whether the disc revolved on the fixed axle or whether the disc and axle, being fixed together, revolved in the frame.

13150. The first alteration made was in the inclination of the disc, its axle always being preserved in the same plane with the vertical axis. As the disc was rendered more vertical, the effect of revolution round the V. axis diminished, and I could not perceive that when it was truly vertical there was any tendency to revolution round the vertical axis, whichever way the disc revolved. But on increasing the inclination of the disc, the effect increased until the disc was horizontal, when it was at *a maximum*. On continuing the successive displacement of the disc, the effect decreased, but it was now the lower edge (being in fact the same edge as before) which moved in the same direction as the revolution round the V. axis. The effect gradually diminished as the disc was made more and more vertical, and when truly vertical there was no effect as before.

13151. Supposing the disc could be sustained by a perpetual revolution on its own axis and then that the frame and disc could be as gradually revolved round a line being tangent to the circle of revolution round the vertical axis, the effect would be as follows. Whilst the disc was vertical, there would be no tendency to move in the horizontal orbit. As the disc became inclined either way, motion in the horizontal orbit would come on in a direction corresponding to that of the outer edge of the disc or the edge farthest from the V. axis prolonged. This would increase until the disc was horizontal, when it would be at a maximum, and afterwds. diminish, to cease altogether when the disc was vertical. Then it would recur again. The motion in the great orbit is always in the same direction with that of the outer edge of the revolving disc.

13152. The next point was to lessen the distance between the revolving disc and the V. axis, the latter and the axis of the disc being always in the same plane. The same effects as before occurred at any distance, increasing and decreasing in amount as the distance decreased and increased. This continued until the vertical axis *passed* through the *center of Gravity* of the disc and was then most powerful, being the most powerful possible when the disc was horizontal, for then the frame revolved with the disc, and if torsion were taken off, it went to the extent that the disc ceased to revolve in the frame, but it and the frame revolved

together, revealing the fact that it is just the friction in the axle which, imparting part of the force in the disc to the frame, sets it in revolution round the point which is restrained, namely the vertical axis.

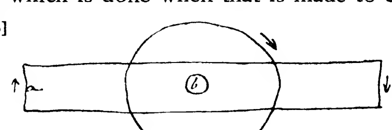
13153. If the frame be carried off on the opposite side of the vertical axis, or if it be carried out in any direction from the V. axis, like effects are produced; the conditions are the same and the results are the same.

13154. When the disc is central, i.e. with its center of Gravity coinciding with the V. axis, it is of no consequence in *what azimuth* it stands—there is no effect of revolution in the frame or the supporting part of the system. If in this position it be carried out parallel to itself in any direction from the vertical axis, still there is no result of horizontal revolution. Again, if the disc and its frame be at a distance from the V. axis, the latter bng. in the same plane as the disc axis and the disc vertical, there is no result of horizontal revolution (13150); but if in the latter case the disc be revolved in azimuth so that its axis shall be always horizontal, then whatever the position, there is no horizontal revolution. What occurred at the center with a vertical disc occurred in every other situation, i.e. cessation of horizontal revolution, so that the various cases become one.

13155. The simple experimental result therefore is this. If with a V. axis a horizontal projection of the revolving disc be made, and if in that projection the moving parts of the disc describe straight lines, then there is no tendency to revolution; but if the moving parts of the disc describe ellipses or circles, then there is revolution, and the revolution coincides in general direction with the motion of those parts of the disc which in plan appear furthest from the V. axis.

13156. Now all is clear. When the disc revolves, it by friction is retarded and soon brought to rest, even if every other obstruction were away. If the frame in which it is held is quite free to move, the frame itself is by the friction carried round with the disc and at last moves with it, the two being as one. This was the case when the disc was horizontal and coincident with the V. Axis, as shewn in this diagram*. But suppose in that case a stop be applied at *a*, which is done when that is made to coincide with

* [13156]



the vertical axis, then according to the well known laws of mechanics, *b* will move so as to revolve round *a* in the direction that *a* before revolved round *b* when stationary, and consequently the center of gravity and the whole frame will revolve in a horizontal direction accordant with the parts of the disc furthes[t] from the V. axis*.

13157. So also in cases where the disc is inclined to the V. axis. There is, in the resolution of the forces communicated by this friction from the spinning disc to the frame, a resultant in every case which is horizontal and at right angles to the line joining the vertical axis with the disc, the direction of which is the same generally as that of the parts of the disc furthest from the V. axis. The resultant is smaller as the disc is more nearly vertical and ceases altogether when it is vertical. Hence there is then no tendency to horizontal revolution.

13158. So this particular part of the Paradox is I think made out.

13159. As another form of expression, we may say that the outer edge of the spinning disc will rotate round its own axis and the Vertical axis in the same direction. If the spinning could be continued indefinitely and all friction or resistance be removed from the vertical axis, it is evident that the revolution round the vertical axis would increase until its angular velocity equalled that of the disc, in which case the same edge of the disc would always be towards the inner and outer edge, the disc would move with the same constant angular velocity, and the disc would become fixed as it were in the frame. Such a result must happen if there were *no resistance* at the V. Axis and *some* at the disc axis.

13160. In respect of (13156), it is clear that if when the system is rotated in free space the advance of *a* be stopped, then the other half of the power of *a* and *b*, that is the power at *b*, will carry the whole system formed round *a*. Being a couple in the first instance, one of the powers is neutralized and the other produces the full effect. Or again, the first rotation of *b* round the center *c* is converted into a rotation of it and *c* round *a*.

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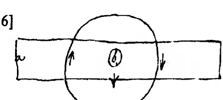
13161. Considering the effort as one always tending by friction to make the frame carrying the disc revolve *with* the disc, the central

* [13156]

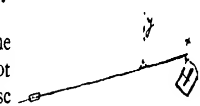


position or that where V. Axis passes through the center of Gravity, and rotation of the disc, should be the most favourable for the result (13152), for then all the power of friction is exerted in turning the frame; none is stopped off by the place of the V. Axis acting as a resistance (13155). This result was experimentally confirmed.

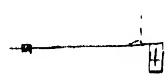
13162. Thus far the results have all been with a chief vertical axis. What happens with the axis in that position will happen with it in any other position, provided the influence of Gravity be removed by counterpoising.

13163. Now as to equipoise of the revolving disc. It was hung as represented, by a hook, so that in all positions the disc remained parallel to itself. Whether the disc was spinning rapidly or at rest, the beam held precisely the same position. There was no disturbance of or combination with Gravity. The same occurred whether the beam was horizontal or inclined in the contrary direction.

13164. Repeated the experiments but with the disc inclined. The result was precisely the same, so that this inclination did not disturb gravity. Another effect however took place: the disc tended to make its frame turn with it according to the law of action before described (13155), so it tended to revolve round a vertical axis at *x*, and when that was stopped by the hook, from then the whole tended to revolve round V. axis *y* as before, the upper part of the edge moving in the same direction generally round its own axis and the vertical axis, because being farthest out from the latter.



13165. Now fixed the disc frame to the lever, so that as the latter changed its inclination the former changed with it, but the lever was parallel to the axis of the disc. No change resulted when the disc was spun. Whatever inclination was given to the lever before hand it retained or resumed, whether the disc was at rest or not.



When the disc was vertical, there was no rotation of it round the V. axis—when oblique as thus there was, but that was simply a case of friction of the former kind (13155), and the lower part of the disc, being the farthest away from the V. axis, went generally in the same direction round the V. axis and its own axis.



13166. Fixed the frame to the axis with the disc oblique. This made no difference with regard to the balance of the lever, so gravity not affected or disturbed whatever position the beam had. But the former friction affect arose whenever the disc was not vertical. So lever being horizontal, it rotated because top part of the disc was farthest from the V. axis. When lever thus, it did not rotate round the Vertical axis.


13167. Fixed the frame so that the disc was at the end of the lever and its axis in a line with the lever. Same results as before, i.e. that whatever position the beam had whilst disc at rest, it kept when disc was in motion, and that whenever the disc was inclined to the V. axis, it rotated round it according to the law before given (13155).

13168. I hung up a bob by the Vertical axis, which was 3 feet long. Could perceive no signs of any deflection from a perpendicular line.

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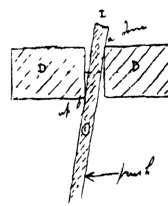
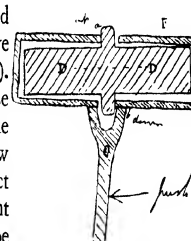
13169. My point of suspension between the V. Axis and the beam is too far above the center of Gravity of the beam, etc. to allow a very small deflecting force to be sensible. So I must give up experimenting for the present and jot down an expectation or two. B. Powell, in the report of the evening, states that if further end of the lever be over poised, so as to bring it down if at rest, i.e. if the disc is at rest, and then the disc be rotated, the lever retains any position it is put in; and instead of the descent of the overpoised end, the lever and weight and frame, etc. begin to rotate round a V. axis, as if the rotation was the equivalent of the descent, but being opposed to it, counteracts it. I doubt this explication. The lever I believe will not rotate if the disc be vertical, and as a proof trial, I believe it would rotate one way if the lever were then tilted in one direction and the other way if tilted in the other direction, because of the reverse inclination of the disc at those times. Yet the overpoised end would retain either one or the other position, being that first given to it.

13170. I believe the sustentation of the overpoise is due solely to the momentum of the disc in a state of revolution, which causes it to tend to retain the same plane of revolution. To change

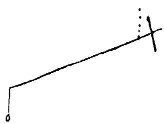
this plane of revolution requires force, according to the principle of Frisi. A small force can only do this feebly with a heavy disc in rapid revolution and therefore changes the resultant plane of revolution very slowly. The overpoise is such a small force, still it produces its effect, which increases apparently as the momentum, i.e. rotation, of the disc diminishes.

13171. What the small force does slowly a greater force, as the push of a finger, does more vigorously. This being applied close to the vertical axis above, represents the gravity force in direction, but it may be applied beneath or on one side equally truly and then becomes Wheatstone's lateral pressure, or includes it. Such a push therefore gives a sense of the strong resistance to change of rotation plane of the disc. When the push is given, there is a powerful tendency to motion of the part of the lever pushed, but the direction of this tendency is to one side or the other of the line of push, i.e. if the push is horizontal, the tendency is up or down according to the direction of the disc rotation. I am not sure which way it is and I have not the apparatus at hand. But suppose for a moment it is friction (or a part of it at least). Then consider the plan, D the disc revolving so that the upper part shall proceed from left to right, and then a push made on the opposite side of the V. axis *c* at ← in the direction indicated. That will transfer the points of friction from the line over the middle of the axis to *a* and *b*, i.e. the rotation being rapid. The friction at *a* will tend to carry *a* downwds.; that at *b* being on the ascending side of the disc will tend to carry *b* [upwards]. This will tend to make the lever *a b* revolve round the line D D in such a manner that L will descend, and consequently, as *c* is a fixed point, the disc itself should tend to descend also, that is move in a direction at right angles to the push. Is it so in fact? (13184). Or being so, should not the fixture of the disc on its axle reverse the effect? For let disc D rotate in frame F, and let a push like the former be applied at ←; the inner or *b* contact would now be on the down going side, and the outer or up going contact would be at *a*; so that the twist, if due to friction in part, ought to be reversed. If due to the composition of force, ought to be as before. Try this.

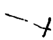




13172*. Have constructed a steelyard about 11 inches long, having the steel axle at one end; a counterpoise at the other; a point of suspension at the end near the axle, so as to be about 0·9 of an inch from the disc when on the latter. When the disc was just counterpoised, the point of suspension, i.e. the axis for that purpose, was very little above the center of gravity, and therefore a sensible beam was obtained; and when set vibrating, returned very well to its position.



13173. Adjusted the counterpoise so as to set the lever inclined, left the disc quiescent but rotated the whole about the V. axis. The position changed instantly and approached the horizontal even when the rotation only slow. This is a very natural result, for centrifugal force will tend to depress the disc end and raise the counterpoise end, and so level the beam. This cause will come into play in all such revolutions round the V. axis, however they are produced, and whether that axis be flexible or fixed. It may often require to be allowed for.

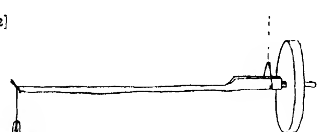


13174. Next, when the disc and beam are still, the latter is inclined. When the disc was in rapid rotation, the beam would remain horizontal if placed so at first, as if against the force of gravity, or it would remain in any other position, as thus, for a time and then begin to move. In these first cases, the counterpoise was but little over and had little force to depress the long end, so when the beam was horizontal, it remained horizontal and there was at first no tendency to revolution round the V. axis; by degrees the disc rose a little—the lower edge projected out and then revolution round the V. axis came on, in the direction of the lower edge round its own axis.

13175. If the fork of the Vertical axis be held stiffly in the hand, so that rotation round it is prevented, then the disc being quickly rotated, whatever position the lever is placed in in respect of gravity, it retains with a power proportionate to the rotation. This is a *simplified effect* and is due to the tendency of the disc from the momentum of its parts to preserve its plane of rotation unchanged, and only to that.

13176. As with a weak rotation the disc rises and becomes inclined, so revolution about the V. axis then comes on according to the first results separated (13155). This rotation tends to set

* [13172]



the lever horizontal again, so that the preponderance of the weight, and its result of Vertical revolution on the other [hand], oppose each other in their effect on the beam. Whenever there is revolution of an oblique beam round the vertical axis, the full effect of Gravity will not appear in the inclination (13173).

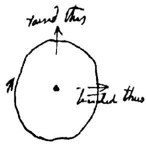
13177. Now made the poise heavier so as to exert a stronger gravity pull upon the lever and raise the disc over the point of support when quiescent, so as to make it nearly horizontal. Then by the employment of rapid spinning of the disc, observed two effects, a first and a following or final one, very different in character. The disc was in rapid rotation and the beam left horizontal and quiescent—the beam tended to keep horizontal, losing that position only slowly, but whilst so balanced the whole revolved round the Vertical axis, not only when the disc was vertical but also when inclined a little. The direction of revolution was the same for both states, but for the inclined states it was in the contrary direction to that described on the former occasion, for in all cases the top edge of the disc tended to go in the same direction round its own axis and the vertical axis.

13178. As the spinning of the disc diminished, it was carried up and became more and more inclined by the preponderance of the weight; then gradually the rotation round the V. axis diminished and when the inclination was a certain amount and the spinning slow by comparison, the rotation round the V. axis stopped altogether and then was renewed in the contrary direction, becoming the phenomenon first described, which then rose to a considerable degree.

13179. So the first effect is best shewn in its true and simple state when the lever is horizontal and the rotation of the disc rapid. When the disc is a little inclined, it becomes mixed with the first phenomenon spoken off, and afterwds. it passes altogether into this effect (13155), the old revolution.

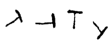
13180. This phenomenon, separated from others, is as follows. Let the observer's eye be at the point of intersection of the Vertical axis and the horizontal lever, looking towards the revolving disc turning watch fashion or from left above to right: then the preponderating weight behind the arm tends to pull its end down and therefore to raise the disc up. If the disc were

quiescent or were allowed to preserve its plane of rotation (13163), the weight would raise it effectually; but being in rotation and also on the stiff prolongation of the beam, it resists ascent; and the new effect comes on, namely a revolution round the vertical axis from left to right, i.e. according with the motion of the upper edge of the revolving disc.

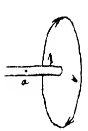


13181*. Looking at the leverage, etc., the pull down by the weight at *r* is equal to a push down at *s* or a push up at *t*, and so becomes the case of force which the whirling globe, Wheatstone's apparatus, etc. shew. So in the case observed above (   ) the result as to direction is that when the disc was forcibly raised, it tended to go to the right hand.

13182. Took the lever and disc off the vertical axis suspension, so now the apparatus consists merely of the revolving disc on a steel axle fixed in the end of a wooden rod. The rod being held at *t t* by the finger and thumb, it was easy to spin the disc very strongly and then by a quick application of power at *t t*, to make the disc revolve about that fixed point either by rising or falling, or to the right or the left; in fact in any direction and also starting from any chosen position of the disc at first. The effects were the same in every case—and exceedingly simple. Of course the rotation of the disc about it[s] own axis was in a plane perpendicular to that of the revolution about the point *t*.



13183. In all these cases, it was very easy to feel first the resistance to displacement of the plane of rotation of the disc (13175). It was also as easy to feel that the resistance was not altogether in the direction of the force applied, but oblique, because of the tendency of the disc to go oblique, or escape as it were sideways in one particular direction dependant upon the direction of rotation and of the extra force applied. Thus when the disc revolved as in the figure and *a* was made the fixed point, then raising power applied as at $\uparrow$ made the whole disc tend to proceed laterally, as at $\searrow$; therefore presents a model for every position.



13184. Now fixed the axle to the disc and held the axle in a fine smooth socket (a glass tube). Every thing occurred exactly as in the former case, so that friction does not sensibly enter into the result, but every thing depends upon the composition of forces.

* [13181]

13185. Suppose the disc a globe, the effects would be the same. Suppose the fixed point *a* to coincide with the center of rotation, then the following effect would happen. Let G be the globe looked at from above, revolving from *a* to *b*; then by the proper depression of the horizontal axis *x*, make the globe revolve round the diameter *a b*, so this second revolution shall be from *c* to *d*; the resultant of the two forces will be a revolution of the ball in the line *e f*; and it is just because the axis *x* must be of course perpendicular to any plane of rotation acquired that the momentum of the ball or disc first resists the change at all, but if that be persisted in, then urges the axle by pressure on opposite sides of the bearings into the new and consistent position. Whether it be the force of the hand or of gravity makes no difference in the character of the result.

13186. If the plane of the disc in relation to the point about which the second revolution occurs is altered, so that a line from it to the plane of the disc is more or less oblique, then like results occur, modified as to amount but all of the same kind.

13187. As to the other or reverse effect which occurs when the disc is much inclined and has only a moderate rotation (13178). It is not merely a rotation round the vertical axis in the contrary direction or according to the motion of the distant edge of the disc as regards the vertical axis, but besides that, the disc tends to set its axis vertical and over the real vertical axis; so that instead of revolving with its frame round the V. axis, it should rise up over it and the V. Axis pass through the center of rotation and gravity of the disc. This is an effect which belongs to the first results and is equivalent to an endeavour of the horizontal disc (13152) to gather in and place itself over the V. Axis in the central position. There friction tends to resolve the two forces into one resultant.

13188. There are three different effects produce[d] which may occur more or less mingled with each other.

The first vertical revolution, 13155, 8, 13149–62, 87.

Opposition to Gravity or retained plane of rotation, 13170, 1, 13163.

Reverse rotation and oblique reaction, 13177, 9–86.

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13189. The *polarity* or the condition of bismuth when in the magnetic field: is it as I think or is it as Weber thinks? Will not the following considerations help to develope this matter?

13190. It is conceded that the magnetic power throws paramagnetics into a polar state, and I suppose Weber and others admit that it throws diamagnetics into their peculiar state of reverse polarity. Now admitting, as I am inclined to do, that the force does make all bodies submitted to it assume a new state for the time, then bismuth will have a state which it gains as it goes into the magnetic field and looses as it comes out of it.

13191. In the case of Iron, the acquirement of this state takes *time*—the Electro magnet takes considerable time to rise to its maximum, and it takes time to fall again. So if bismuth and phosphorus assume a new state, *time* will probably be concerned there also.

13192. If this state exists, it must be one favouring the *transmission* or *conduction* of the magnetic force. It is so in the case of iron, and it would seem unnatural that it should not be so in the case of bismuth.

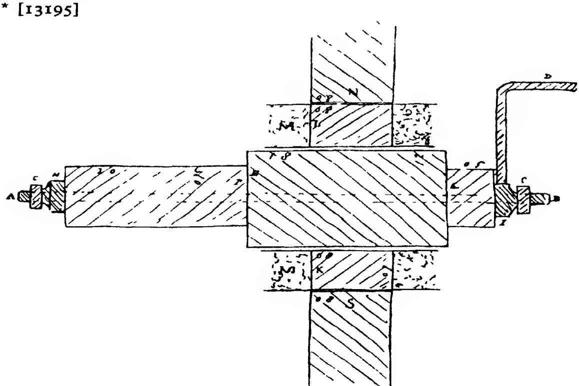
13193. But if the supposed reverse polarity be occasioned by the magnetic force, it must be the *same* as the state set up; and it would be equivalent to *setting up* a resistance, not to the removal of one. Therefore the assumption of a reverse polarity, and the assumption of a state which tends to diminish the resistance to magnetic conduction, are in opposition to each other and *both cannot be true*.

13194. It seems possible to determine experimentally which of these is the truth of nature upon the following principles. Suppose a piece of iron between two magnetic poles: it becomes polar, i.e. acquires its state. Suppose it turned half way round; the state it retains will be adverse to the magnetic conduction, but then it will in time acquire the new polar or peculiar state which is favourable. If it be again turned half way round, the same sequence of changes will occur. So that as the iron moves round, it will become less favourable to magnetic induction, and the

lines of force across it from Pole to Pole of the magnets will open out and pass more through the surrounding air or other body occupying the magnetic field; and as it is resting and rising to its final state, it will conduct better and the lines of force will draw in and concentrate within it. This alternation of the lines of force which *must* accompany the change of magnetic conduction power are well fitted to prove experimentally the condition of the body: if only the currents which the changes are competent to produce can be gathered up by proper commutators and sent on to a galvanometer.

13195*. I have had an apparatus constructed of which the general plan and measures in inches are here given. A B is a copper rod serving as an axis, held in holes at the top of the two supports *C*, *C*. D is a handle by which it can be revolved or moved to and fro. E is a cylinder, solid, of Iron, bismuth or any other body to be subjected to an experiment, 1·8 inches long and 1·2 inches diameter; F and G are cylinders of box wood. H and I are two nuts, and as a small hole runs through the center of the cylinders E, F and G, these nuts hold all three firm together, especially when double discs of sand paper are introduced at F and G to give holding friction. The supports *C*, *C*, hold the axis 1·2 inches from the base. K and L are two cylinders (short) of soft iron each with a helix of fine wire M round it; one of these, K, contains 63·7 feet of wire and is 1·8 inches in its external diameter; the other, L, has only 32·3 feet of wire and is 1·3 inches in diameter; the wire is copper of inch in thickness. These pieces of iron are blocked up in their place so that one is on each side of the great cylinder E, their axes being at the same level but at right angles with its axis.

* [13195]

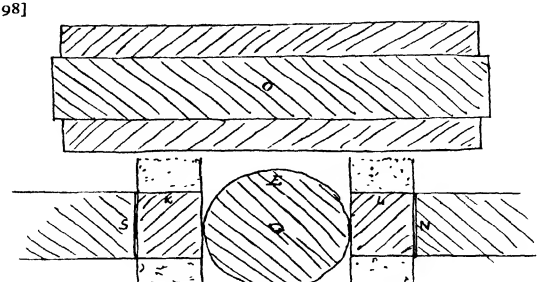


13196. Two bar magnets S and N, cylinders, each 0·8 in diameter and 8·5 inches long, well magnetized, were placed at the same level with the soft irons so as to form magnetic continuations one with the other. Under these circumstances, the soft iron pieces formed the N. and S. terminations of the magnetic poles which were acting across the space occupied by the experimental cylinder E. These bars were held and fixed in their place so as not to be easily moved.

13197. Now the idea I have is that the lines of force proceeding from one pole to the other across the three cylinders K, E and L may, as before said, be concentrated or diffused through E at *given times*; and will of course have a like disturbance in K and L at the same times, some of the lines of force which go through K, L and E at one moment, being diverged and sent through the air at the next moment, to return to their first position at the following moment, and then again sent outwards; and that as these changes would not depend upon any change of place of the helices in relation to the magnetic poles, they would *prove* the displacement of the lines of force by the *position and time* of the experimental cylinder only. Being gathered up by the Galvanometer and commutator, they would have to shew this change.

13198*. As the sum of Magnetic force from N to S would be always the same, I concluded that any lines of force dismissed for the time from the cylinder E would pass through the surrounding space. With the idea of gathering some of these up, I had a cylinder of soft iron O prepared, 0·85 inches in diameter and 4 inches long. Covered copper wire like that round the pieces K, L, was formed into a helix round this iron; the wire was of an inch thick and there must have been about feet employed. It was supported above the other helices, etc. and its ends overlapped the S and N ends of the magnets about 0·5 or 0·6 of an inch. The idea was that, of the lines thrown out of the

* [13198]



cylinder E, enough would go into the iron so as to produce an effect upon its helix, which might upon occasion be added to the effect of K and L.

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13199. The Galvanometer I have is Rhumkorf's. I have put on a new Cocoon silk and thus got rid of much torsion, and it is now in Excellent condition. It is about 4 feet south of the apparatus and its magnets—all is well and steadily fixed.

13200. The experimental cylinder E being *soft iron*, and the two pieces K and L with the magnets in place, the wires were so joined that any exaltation or any depression of the states of K and L should combine to send an electric current through their joint wires. Then the piece of iron O was put into its place above, and its wire conjoined to the end of the former wires, so that any *fall* in power in it should coincide with the *rise* of power in the pieces K, L, in sending an electric current through all the wire. The ends of this long series of wires were connected with a commutator and that again with the Galvanometer. In order to test the various connexions, a thermo electric generator was made with a piece of platinum and a piece of iron wire. This being introduced at one of the junctions and warmed by the fingers, sent a current through the whole, deflecting the Galvanometer needle permanently 10° or 15°.

13201. Suppose the Experimental cylinder E to revolve through 180°—then stop an instant—then to return back through 180°—then to stop—and so on continually. On starting it will, according to the supposition of change of state, time, etc. (13191–7), cause a current through the wires in one direction. On coming to rest and stopping, its state will rise and a current will be caused in the other direction. On starting back, a fresh current *like the first* will be caused—on stopping, another current like the second; and these contrary currents will be renewed every time the cylinder moves and comes to rest. Now a commutator which changes after the cylinder has come to rest, before it starts again and also in the middle of its motion through the 180°, will gather up all these currents into one continuous current, and if this affects the galvanometer, *then* the research by experiment is open.

13202. I made this change by hand (having a cork handle to the commutator) and therefore very slowly compared to what it ought to be, but I am happy to say the Galvanometer moved.

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13203. The motions were repeated. The effect always followed and in the same direction—the needle constantly went to one side. It was even evident with two moves only.

13204. If the commutator were not changed, the effect did not occur. So that it did not appear to be the result of mere shake, making the magnets approach towards the central cylinder by attraction.

13205. In order to distinguish which was most powerful, either the end pieces of soft iron K, L, or the larger top piece O, I turned the latter round end for end, keeping the connexions as they were—this ought to reverse its action, so that it should no[t] oppose the action of the end pieces; but the result was the same as before, being in the same direction and I think as strong—as if the top piece O did little or nothing. I then removed it to a distance, leaving it still in the circuit. The effect was the same as before.

13206. Then I removed it out of the circuit altogether and set it aside; and now the end pieces only could aid in sending on a current to the galvanometer. The effect was as good and in the same direction as before.

13207. I turned them both in their places end for end, leaving the connexions untouched; and now the effect at the Galvanometer was produced, but in the *reverse* direction.

13208. So there is every reason to believe that I have the effect I am looking for, an iron cylinder E being employed. Now if I can make the apparatus sensible enough to shew the effect with bismuth or phosphorus, then the following results will ensue.

13209. If the state assumed in *time* by bismuth makes it a better conductor than before (13192), then the current it may give will be in the same direction as that obtained with iron, and its polarity or the equivalent to it (the polarity of the lines of force) will be like that of iron. If its polarity, or the state assumed in the

magnetic field, be the *reverse* of that of iron, then the current it may give will be in the *contrary* direction.

13210. The non action of the cylinder of iron (13205, 6) would seem to shew how much of the external magnetic force must be disposed of through the air and space around, since the change in that part of space occupied by the iron is insensible. *All* the effect of the change in place of the lines of force seems to be shewn by the helices of the terminal pieces, and but little of it by this piece outside, only its own small fraction as a favourable but small part of a large acting space.

13211. In order to exalt the effects and make them available for bismuth, etc., first I must have a *new commutator* acting quickly and conjointly with the motion of cylinder E (   ).

13212. Next a stronger Magnet: even the Logeman magnet. Perhaps the soft iron need not touch the Logeman, but being close to the bismuth, may be distant from the Logeman poles—or else may connect them by soft iron bars.

13213. Whether a field between large faced poles or between pointed compressed poles will give the best effect must be tried by experiment.

13214. May have larger soft iron pieces and more wire on them, and then may go further off from the Galvanometer.

13215. With much fine wire on end pieces, may use Dubois-Reymond's Galvanometer.

13216. Try also end pieces with thick wire round them and the thick wire Galvanometer.

13217. Effect of extent of Arc of vibration of exp. piece E.

13218. Effect of Velocity of change of place.

13219. Different substances.

13220. A substance which settles nearly at the Zero state, and so seems to be then indifferent, may not have that for its natural state but may shew an induction effect. Also the time for each may be different, although the final state nearly the same.

13221. Consideration. If bodies (and all bodies) *assume* an induced state, then they will assume it independantly of the medium which surrounds them. Thus solution of Sul. Iron will assume it just as much in a magnetic field with air or with like solution of the Sulphate of iron (except for a small difference). But then,

what of Space? How can space assume a new state? Yet space must partake generally of the condition which is general to Para and diamagnetic bodies, and especially if the condition conferred is in the same direction for both—which it will be if in both case[s] a greater facility of magnetic conduction is the final result.

13222. In such a point of view, one would rather expect that the change would be in opposite direction, i.e. as paramagnetics become more paramagnetic, diamagnetics would become more diamagnetic—but then this would not seem to accord with the idea that the change always favoured the communication or transference of the force (13192). Experiment must instruct us.

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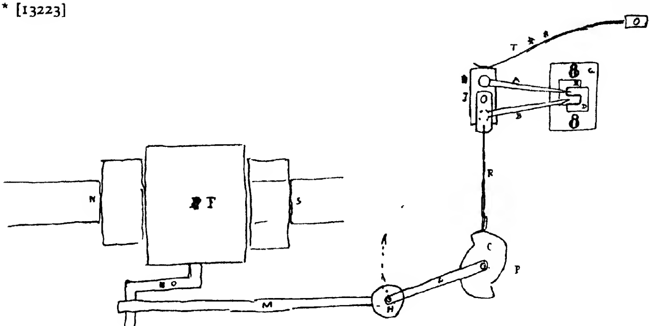
13223. I have had a commutator associated with the apparatus described at (13195), which will be understood by the above rough plan*. P is an upright axle, carrying midway the double clam C, and at the top the arm L and handle H. This handle, moved in the direction of the arrow, moves all the other parts; for as F is the experimental cylinder of iron or bismuth, with its helices, magnets N, S, and handle O, and as this handle is brought upwards and connected by a light deal rod M with the arm L at H, so, as H is carried round, it serves as a crank and gives a to and fro motion to O and the cylinder F. If the arms L and O were of equal radius, then of course each would describe 180° or any equal amount in the same time, but if O be made longer than H, any smaller arc, as 120°, may coincide with 180° of H, and then the cylinder F will rotate backwards and forwd. through 120° as H continually revolves. This 120° for F is substituted for the 180° of the former experiments (13201).

13224. Then as regards the Commutator: I is a sliding piece of boxwood carrying the two spring contact pieces A and B, and

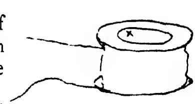
* [13223]



also the thin stiff plate rod R whose rounded edge bears upon the edge of the clam C and is kept up to it by the spring T; so as the clam C turns, it gradually carries the stage I outwards and then suddenly let[s] it inwards again, and this it does twice in every revolution. E and D are two pieces of brass plate, not touching each other, fixed on a box wood stage G which can be shifted by the hand to and fro to the extent allowed by the two holes and the pins in them. Now as the depth in the fall of the clam C is equal to the width of the branches of D and E, so it is easy to see that the commutator changes twice in every half revolution of the handle H; for at the falling in of R the conductor B will pass on to D suddenly and the arm or conductor A on to E, but as the handle goes on, it will gradually carry the piece I back again, and if properly arranged, will at 90° (or at any other time if required) pass the conductors back in to their first relation to E and D. There is this still further power. If by a number of revolutions of H a current has been gathered up and sent on to the galvanometer, swinging the needle in one direction, and the needle begins to turn back, then by carrying the stage G by hand from the spring the required distance (allowed by the holes and pins), the collected current is changed in direction and so the effect of accumulation at the galvanometer still goes on.

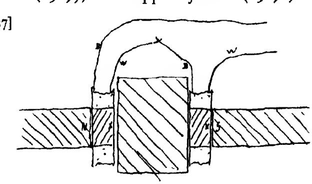
13225. The clam C is held on the axle P by two nuts so that it can be fixed in any position in respect of the arm L, and therefore the changes at the commutator be made to coincide with any period of the motion or state of the arm O, and consequently of the experimental cylinder F.

13226. I verified the relation of the helices, making one end of the helix wire black B, the other being left white W; and then when currents were sent through them, marking one (and the like) end of each helix iron with a cross.



13227*. Now arranged the apparatus, leaving the experimental cylinder of iron (13200), putting the two short irons and helices and the magnets into their places in the order indicated, connecting the W wire of one with the B wire of the other, and the remaining ends B and W with the commutator and that with the Ruhmkorf's galvanometer (13199). The upper cylinder (13198) was removed

* [13227]

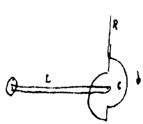


and out of action. The thermo-current (13200) applied at one of the junctions shewed a good contact and circuit through the whole.

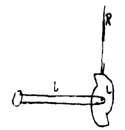
13228. Have arranged the commutator so as to give the quick change just before the experimental cylinder began to move from its point of rest or return (13224), and therefore the slow reverse change in the quickest part of its motion, for this was assumed as the best condition for the expected effect; but I found that on working the handle and apparatus–*little or no effect* was produced at the Galvanometer.

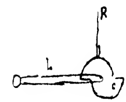
13229. Proceeding to examine first the joint action of the soft irons and their helices, I changed the connexions there, joining W and W together and sending B and B to the commutator; and now the best effect came out and indeed a very fair one, for 20 revolutions of the handle H sent the galvanometer needle aside 5° or 6°; and then by going on and changing the piece G (13224) and so accumulating the effect, the swing became very good and makes me hope I may succeed with bismuth. I must develope this effect of position hereafter (13230), accepting it for the present and working on for other points.

13230. *Position of the commutator.* By the clams the commutator can be made to change at any period of the motion of the Experimental cylinder. The quick change at the fall is 90° from the slow change for one revolution of the handle H, there being two quick and two slow changes for a whole revolution. One revolution brings the experimental cylinder over to a state of rest and then carries it back to a state of rest; and each of these journeys, beginning from a state of rest, increases in rapidity to the middle and then diminishes to a state of rest. The journeys, whether beginning on the one side or the other, are supposed to be exactly alike in their effect, and the commutator action is absolutely the same for the two. The quick commutator change is the most definite and probably the most influential in separating the numerous feeble induction currents that I desire to bring up; therefore I proceeded to make changes in it and mark the effect. These I will indicate thus. Suppose the clam C fixed on the axis of the arm or crank L as in this figure; then it is clear that the quick change at the commutator will occur at the instant that the experimental cylinder



is brought to rest. If L were in the other half of its revolution, the effect will be the same. I will therefore say that a quick commutator change occurs at 0° from a stop of the experimental cylinder. Suppose the clam fixed as in this figure, then the *quick change* will occur at about 25° (of the motion) of the arm L after the Exp. cylinder has begun to move. If the adjustment of the clam be as in the third figure, then the *quick change* will occur at 90° or when the motion of the experimental cylinder is the very quickest; and so it may be made to occur at any time of the movement of the mass experimented with. The effect of these and such changes were tried for both states of the piece G of the commutator with the following results. *After* means after the moment of stopping.





0°	after–	very good–	needle	left . . .	very good–	right
5°	,,	very good	,,	left . . .	very good	right
10°	,,	fair	,,	left . . .	fair	right
20°	,,	little	,,	left . . .	good	right
30°	,,	nothing	,,	0	fair	right
40°	,,				fair	right
50°	,,	little	,,	right . . .	very little	right
70°	,,	fair	,,	right . . .	nothing	0
90°	,,	good	,,	right . . .	fair	left
110°	,,	middling	,,	right . . .	pretty good	left
140°	,,	nothing	,,	0	pretty good	left
150°	,,	little	,,	left . . .	nothing	0
160°	,,	fair	,,	left . . .	little	right
170°	,,	excellent	,,	left . . .	better	right
180°	,,	very good	,,	left . . .	good	right
185° or 5°	,,	pretty good	,,	left . . .	very good	right
190 or 10°	,,	fair	,,	left . . .	fair	right

All these results were obtained with 20 like revolutions of the handle H, and therefore with 40 journeys of the Exp. cylinder F.

13231. Now the two positions of piece G ought to give effects of like amount in opposite directions, but that is not fully the case. At the beginning and ending of the journeys, the effect is pretty well, for 160°, 170°, 180°, 0°, 5°, 10° and 20° accord in results and power moderately well. At 40° and 50° they are absolutely opposed, the amounts being however small then; and on each side of these, though not opposed in direction, they are in amount of effect. The same or something like it occurs at 140°

and 150°, as was to be expected; 150° is almost nothing and corresponds to the position of change of direction; 50° is the other correspondant position of change or is near it.

13232. The want of accordance in the two positions of the piece G probably depends upon the circumstance that the change does not place the parts E and D in exact like relation to each other–such a circumstance will make little difference in the moment of quick change, but may considerably affect the moment of slow change, and irregularities in the form of the clams in the gradual part may have some effect also in this matter.

13233. However, as the effects are the same on both sides of 0° and are also greatest about that point, so that is the position of the clams which gives the best results with the commutator.

13234. Leaving the clam adjusted to 0°, and the irons and helices in their places as described (13229), with like wires, i.e. W and W joined together; I took out the smaller iron helix L, and found that the other acted in respect of direction like both but weaker. I then turned iron helix L 180° round and restored it to its place–it opposed the action of the other, K, and there was no sensible action with 20 revolutions of the handle L (13223). I then turned it back into its first position and now had the effect of both as before.

13235. Call the weaker helix L and the stronger K (13195).

13236. By movement of Exp. cylinder, had the effect of K and L–the needle went to the right. Changed connexions only, so as to join wires B and W of helices together and the other B and W to Galvanometer–scarcely any effect now, but what there was was to the left, as if the stronger helix overcame.

13237. Removing L from its place caused the Galvanometer needle to go to right–and then removing K from its place caused the same thing, shewing that as now joined together, B to W, they acted alike by like removing action. Putting L into place, the needle went to the left–being of course the reverse action of taking it out. Movement of the cylinder F with L only in place–scarcely any effect at the needle–it is the weaker iron and helix. Taking L out again–then the needle to the right as before. Putting K into place–then needle to the left, accordant with its action when taken out. With movement of the cylinder F, a little

motion of the needle and to the left. Now L was again put in its place, sending the needle left as before.

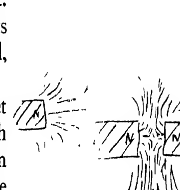
13238. But when the cylinder F was worked with both K and L thus in, there was nothing scarcely–a trace to the left. Turning K, or the stronger, 180° round in its place and then working cylinder F, there was a little action and needle to right. When K turned back and the cylinder F worked, there was a little action and the needle to the left. All this time W and B of the cylinders have been joined together.

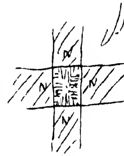
13239. Now *joined white of* L *to white of* K–worked cylinder F–still only a feeble action and to the right. Tried again after swinging and clearing the needle–good–perhaps as good as before when thus conjoined.

13240. The removal of L sends the needle to the right and the removal of K sends it to the left, shewing that the two are opposedly joined together as regards their wires. Turned K 150°–then putting it [in] sent needle to the left–L put in without turning sent needle to the left also–all this consistent, but helices are now in like position. Then moved the exp. cylinder and there was a little effect to the left, as if the two *opposed in power*; turned K back to its opposed or anomalous position and now by motion of the cylinder there was a *good* effect to the right as if both now conjoined. This discrepancy must be worked out somehow.

13241. The irons and helices are now in the anomalous position (13229). Withdrew the magnet N a little way–the needle went *right*. Withdrew the magnet S a little way–the needle went left. On approximation the reverse effects occurred. So these effects are opposed to those which occur when the cylinder F is worked, the magnets remaining stationary.

13242. In reference to another point. A north pole of a magnet has lines of force issuing from it which are polar. If two north poles are opposed to each other, the lines of force have the form indicated. But suppose four N poles–the lines (what few there might be) would proceed to the middle and then rise upwds. and downwards. But suppose further, six north poles inclosing a cubical space. What would be the condition of that space? What would a magnetic needle do in it? Probably nothing. What would a moving wire or revolving ring do in it? Would it have

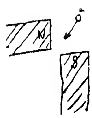




any state, as a state of tension; and as the magnets should receed from it or open out any way what would be its state and how would it come on?

13243. There could be no lines of force in the inclosed space. Can it have a state of tension distinct from the polar or current state, and is it that I tried to find by analogy with static electricity—a state of magnetic tension without polarity. In that case we ought to find a quality by which to recognize it. Would it affect the Electro chemical state and so give voltaic terminations?

13244. The inside of a hole in the end of an iron core ought to have a like state. Perhaps two holes at the ends of a core might serve as chambers for experiments.



13245. How would a piece of bismuth behave there? If N and S were two poles of magnets, a piece of iron allowed to move along the oblique line would go from *a* towds. the angle more strongly than if one magnet away. Then a piece of bismuth ought to tend to go from the angle. If the two poles are both N or both S, the piece of iron should tend to go out, I think, and a piece of bismuth tend to go in; for the angle would partake of the character of the inclosed space before spoken of, i.e. the spot *b* would have weaker magnetic power than any where else.

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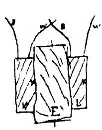
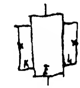
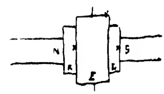
13246. In the following figures (relating to expts. like those recently described), N and S are the magnets and the north and south poles of the magnets (13227), K is the iron with the stronger helix, L the iron with the weaker, i.e. smaller helix (13235). The white wire of one shall be constantly connected with the black wire of the other, and the change shall be made by putting them accordant with the polarity of the magnet poles, or reversing one so as to bring about the anomalous or opposing condition. When they are accordant they will stand thus: when anomalous or opposing, thus.

13247. The two soft irons (13195, 235) were arranged on each side the iron exp. cylinder E, and connected with wires W and B together; the other wires went away to the commutator and so on to the galvanometer. The thermoelectric test shewed a complete circuit. Double movement of E 20 times—gave no effect. So the soft iron, etc. without the magnets gave no sensible current.





13248. Turned K round 180° into the anomalous position, and then gave 20 double movements, i.e. 20 revolutions of the handle L (13222[1]). The needle was not sensibly affected. So again *the irons without the magnets do nothing*. Returned K to the first position (13247)–movement of E–no effect.



13249. Put S magnet into its place–this sent the galvanometer needle to the *left* (no movement at present and the commutator kept in one position). Put the N magnet into its place and this also sent the needle *left*. Withdrew S magnet- needle went to *right*. Withdrew N magnet–the same thing happened. So that both magnets accord and both helices to these approximating and receeding action[s], the helices, etc. K and L being in *accordant* position. Found also in confirmation that when N and S either close up simultaneously or were removed simultaneously, the effect was to the *left* and great, being the sum of the former effects. So much for approximation or recession of the magnets N and S.

13250. Now examined *the effect of displacing the helices K and L*, leaving the magnets and irons untouched. Took out K–the needle went to the *right*. Took out L–the needle also went *right*. Put in L–needle went *left*. Put in K–needle went *left*. So these actions are accordant; and whether we take away magnet N or helix K, the effect is the same, the needle goes to the right. Removing S or L produces same effect. Putting up S or L has like effect, and the approaching N or K is alike as to current produced. This should be so.

13251. Now turned K 180°, so as to bring it into the best condition for the production of a current by movement of the iron E, i.e. into the *anomalous position*; and then again verified the effect of approximation and recession. Thus:

Withdrew	magnet	N	needle	went left
,,	,,	S	,,	,, right–weaker than before because L is small.
Approached	,,	N	,,	,, right–strongly, K being the larger helix.
,,	,,	S	,,	,, left–moderately.
,,	,,	N and S		,, right–being the difference of the two helices.

All this is accordant with the reversed position of K.

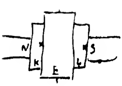
13252. Now as to effect of removals of K and L helices:

Took away	K–strong action–needle went to	left
Restored	K–Do.	right
Took away	L–fair action	right
Restored	L–Do.	left

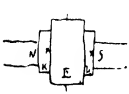
[1] Should be 13223.

All this accordant with itself and with the former results. Whether it was the magnet or its associated helix that was withdrawn, the effect was *the same*, and a like result was produced by the approximation of either. But the actions on one side of the cylinder E were the *reverse* of those on the other, because the helices are in the *anomalous* position.

13253. Now the exp. cylinder E was moved, the magnets being retained permanently in their places and the helices also. The motion was as before for 20 revolutions of the handle L (13222[1]), and the needle went to the *left* hand. Repeated the experiment; again the needle went to the *left* hand. The commutator was now moved over by hand, i.e. the part G (13222[2]), and then movement of E gave a deflection of the needle to the *right* hand, once and again. To prove that the two helices were acting together and the effect the sum of their action, K was turned 180° so as to be *consistent*; and then movement of E gave a little trace of action, needle to the *right*–and then changing part G of the commutator, movement gave the least trace to the *left*. This effect therefore is the real *difference* of the two helices, and when they were in the anomalous position, we had the *sum* of their effects.



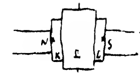
13254. I am not sure that the clam has been in the best position or even well fixed (13230), so now I have fixed it so that the quick passage one way should be the best, being a few degrees after a stop of the experimental cylinder (13230), and the following is a repetition of part of the former results, the magnets being in place and the helices *accordant*. Moved E by 20 rotations of the handle–very little effect to the *left*. Passed G part of commutator over–then movement of E gave very little effect to the *right*. Returned G to the first position–then movement gave very little effect to the *left*.



13255. Then removed the smaller helix L, and now by movement the needle went well to the *left*, shewing that K had had to overcome the opposed action of L while it was in place. Took out K and put L in as before, and its effect alone (by movement) was to the right.

13256. Placed K and L in the *anomalous* position. Now the movement of E for 20 revolutions of the handle gave a fine

[1] Should be 13223.



deflection to the right equal to 5° nearly—and by alternating the piece G with needle swings obtained a swing up to 8° or 9° on each side of Zero.

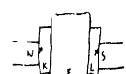
13257. It is quite clear that when K and L are *opposed* in their action as respects effects produced by the movement of the exp. cylinder E, they are *accordant* as respects effects produced by approach or recession of the magnets N and S. In following this out and explaining it, it is to be remembered that there is motion of the commutator in the one case and not in the other—but as the commutator changes four times in one revolution of the handle, its influence does not as yet appear to me.

13258. Being in the *anomalous* position as above (13256) and therefore best for movement action, I tried in confirmation effects of removal, etc., the commutator remaining fixed the time.

Removing N	sent needle well	*right*	all alike, as ought to be
Removing K	Do. . . .	*right*	
Removing N and K	Do. . . .	*right*	

Removing S	sent needle well	*left*	all alike, as ought to be
Removing L	Do. . . .	*left*	
Removing S and L	Do. . . .	*left*	

But the two sets of effects are opposed in direction to each other.



13259. Now for effects of movement of E, the position of K and L when in being *anomalous* as before.

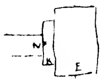
S and L left alone (N and K being taken away). Movement of E for 20 revolutions of the handle gave a little deflection to the *right*.

N and K left acting alone (S and L being taken away). Movement of E for 20 revolutions of the handle gave a good deflection to the *right*.

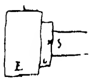
S and L and also N and K being in action at once gave, as was to be expected, a joint and large effect of deflection to the *right*.

13260. So the effect of N and K is *by movement* to the *right*—and also by *withdrawal* of the magnet or helix to the *right*—both effects are *alike*. The effect of S and L is by movement to the *right*, but that due to withdrawal of the magnet or helix is to the *left*. So here is the point of difference, and if we speak of the effect of approximation, then the difference is transferred to N and K,

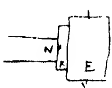
for S, L, are alike in effect by movement or by approximation. Develope clearly this relation of difference.



13261. Made the commutator a fixture and placed N and K thus in the anomalous position but without S or L. *Withdrawing* N and K sent the needle to the *left*, and *approaching* them to E sent it to the *right*. The experimental cylinder of iron E was turned into different positions so that any polarity it might have might shew its effect, but there was no difference produced by that change of E.



13262. Then S and L were put up to E alone, as represented. When *withdrawn*, the needle went *right*; when *approached*, it passed *left*, and that in every position of the iron cylinder E. So that E acts as if in no way polar, but as soft iron, and the actions of K and L, which are in contradictory positions, are in their effects contrary. These effects were obtained again and again.



13263. Now with movement only of E. The joint action of N and K sent needle well to the *right*—and with like movement only of E, the joint action of S and L sent the needle also to the *right*. So that still there is the contrast as to the results—movement gives like results when approximation gives opposite results.

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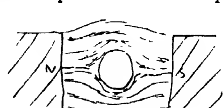
13264. *Magnetic Polarity*. Perhaps have a new paper on this point. In which case take up (Exp. Res., 2822) and all that part and reconsider and enlarge it.

13265. Compare a sphere of bismuth and one of iron when both are stationary in the magnetic field as to their supposed polarity—then rotate them and consider the currents out of them and their indications of polarity (Exp. Res., 3164, etc.).

13266. Compare a sphere of soft iron, one of bismuth and one of hard steel, magnetized, when in the magnetic field and the latter reversed in polarity. Will not the hard steel on revolving give the current due to its own lines of force and be *unlike* the spheres of iron and bismuth revolving, thus shewing the difference of its polarity from them, and the *sameness* in character of their polarity?

13267*. A sphere of hard steel unmagnetized. When revolved, will it not be unable to produce currents if quite untouched by

* [13267]



the lines of force of the field around—and would not a little magnetic needle shew the deflection of the lines? The steel should not be polar. Perhaps it ought to be small or be made of discs of very hard steel plate. Is too Paramagnetic (13421, etc.).

13268. A shell of steel as a cylinder would hardly do, because the space within would convey lines of force. Would give an excellent variation of experiment.

13269*. A sphere magnet in the magnetic field either accordant or discordant. If revolved, will it give its own current and that the same as if revolved with the magnets N and S away?

13270. The reverse magnet should be as bismuth—if bismuth is the reverse of iron; if it gives results contrary to bismuth, then bismuth is polar the same as iron. Examine all these bodies by revolution in the magnetic field.

13271. Polarity references, Exp. Res., 2825. See also 13242–13245, the indications which would be given by a *crystal* of bismuth in such opposed and complicated magnetic fields.

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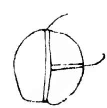
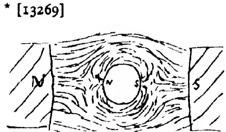
13272. Set a hard sphere of steel revolving in a magnetic field. If *no* lines of magnetic force pass through it, then no current will be produced in it (Exp. Res., 3097). Perhaps even if a copper disc or wire were introduced equatorially there might be no current, because of the quelling power of the steel over the included space, i.e. the *excluding* power of the steel. If so, then contrast the effect with a sphere of soft iron—and from that proceed to a sphere of platinum—of copper—of bismuth, etc.—and so up to hard steel—hard steel and soft iron being the extremes of the series.



13273. Probably hard steel will present that most desired *medium* or *space* of *no magnetic power*. It may claim the right of being a "nonconductor of magnetism". If so, it will present a striking contrast and relation to mere space or a vacuum—and just as striking a relation to iron as its antipodes.

13274. If hard steel a non conductor of magnetism, then a sphere of it in the magnetic field (of moderate force) ought to deflect the lines of force about it (13267), and they ought to pass round

* [13269]



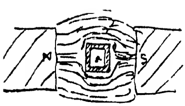
instead of through it. Then a small magnetic needle ought to shew this deflection and contrast with the effect with iron*.

13275†. Again, a plate of hard steel there, as a button, ought to send the lines of force parallel to its sides, and this should be shewn by a small weak magnetic needle and be in full contrast to the effects with iron and half cont[r]ast to those with copper or space.

13276. If a cylinder, prism or plate of steel, hard, were in the magnetic field, then filing on a plane midway ought to shew the same state of things and very well too.

13277. If all this true, we may liken the magnetic field to a *flood of power* which, when an obstruction comes in its way, as a block of hard steel, rushes round it and is conformed to it, just as happens with a flood of air or water. We may perhaps even reach to interference of its lines of force in the places analogous to the eddy places of material floods.

13278. If hard steel a non conductor of magnetism (13421) then it may keep out magnetism, and a flood of other illustrations and proofs will arise. Thus:



13279. A box of steel plates ought to shew *no magnetism* at *a*. A small needle should not point there.

13280. Filings should not arrange there.

13281. A bismuth crystal should not point there.

13282. A rectangle or ring revolved there should give no currents.

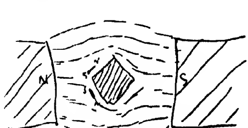
13283. Little balls of iron should not attract or repel or affect each other there.

13284. It should be as if N and S were away. Such a chamber would be free even from the earth's lines of force (13421).

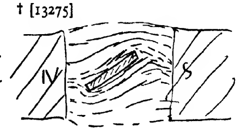
13285. Fine contrast and likeness between Steel and iron here. Needle not point in hard steel because steel excludes the power from passing through itself–not point in soft iron because the iron gathers to and conducts all the power through itself. See former error as to hard steel, Exp. R., 3292.

13286. Contrast of the iron and the steel as of a *conductor* and a *nonconductor*. It coincides with the contrast of Paramagnetic and diamagnetic bodies, and presents the extreme case of such substances.

* [13274]



† [13275]



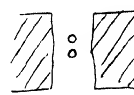
13287. Hard steel may be examined in different ways, as:
by a small magnetic needle, when in the magnetic field.
by its movement or the movement of a wire connected with it or acting about it.
by its action in media–its differential action in relation to them.

13288. Hard steel ought to be expelled out of the magnetic field both by Para and diamagnetic bodies, i.e. if it be a non conductor and so more diamagnetic than any of the rest.

13289. It would be very odd to see it sent out by sol. Sulphate of iron, solutions of nickel, etc.–these being paramagnetic bodies; and just as odd to see it sent out in water, Sulphuret of carbon and diamagnetic bodies.

13290. Hard steel and bismuth on differential balance–bismuth ought to go in as if magnetic.

13291. Hard steel and iron in field of equal force ought to attract each other equatorially, as iron and bismuth do, but far more strongly, though for the same reason (Exp. Res., 2817, 31).

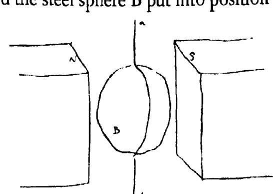


13292. Two hard steels ought to repel each other, as phosphorus does (Exp. Researches, 2814, 5, 6, etc.) and for the same reasons.

13293*. If a block of steel displaces the lines of force (13421) and not merely annihilate or otherwise dispose of them, then the moving wire thus applied will prove many things. Let N and S be two magnetic poles, B a sphere of hard steel and *a b* a covered copper wire partly in a vertical plane which would pass through N and S but partly bent to adapt it to the form of the steel sphere. Suppose the sphere in its place; the removal of the wire *a b* outwards would intersect half the lines of force passing between the magnetic poles, for those displaced by the sphere B on this side would be intersected by it. Then suppose the wire *a b* returned to its place and the *sphere B remained*–after this a like removal of the wire would not intercept the same amount of lines of force but *less*, in fact by so much less as would amount to half the lines of force now passing through the space before occupied by the steel B.

13294. A like effect should be produced if the wire *a b* were left in its place and the steel sphere B put into position or taken away.

* [13293]



Opposite currents ought to be induced in the wire *a b* by this action.

13295*. A like effect ought to be produced thus. Let N, S, be the magnetic poles and B the steel sphere, and let *a b*, instead of a mere wire, be a ring helix. Then if it be a fixture and B be approached towards it, it by throwing the stream of power outwards before it will throw the lines of force across the parts of the helix and produce probably a strong current, to be followed by a contrary one as the steel receeds. Or if the steel be stationary in the midst and the helix be brought towards it, the contrary currents will be produced.

13296†. Or have a large field of equal magnetic force and a smaller ring helix than that above, so small indeed that as it is carried parallel to the magnetic axis from Pole to pole, it shall shew little or no current, because it *intersects* few or no lines of magnetic force. Then let B be a sphere of space or a neutral body (water will do, it is so little diamagnetic) and moving *a* to or from it on either side will shew no effects, because the lines of force still go on as before, parallel to each other. Now let B be a ball of iron; then movement of *a b* will give currents, shewing that the lines of force converge upon and enter into and through it. Then make B a ball of hard steel, of course unmagnetized, and then motion of *a b* will shew that the lines of force diverge away and are driven from it because it cannot conduct the power.

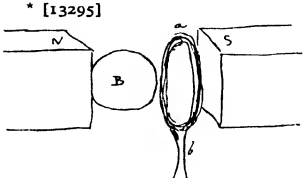
13297. Does not or will not all this shew that the lines of force are there, that they can be curved and bent about, displaced, etc. and have a real physical existence; for the power which the steel drives from one place is found in another by the moving wire, which *cannot* be said to induce it—the *whole amount* of power in the media between the poles being *always the same*.

13298. Will not these experiments prove fine illustrations of the power of the moving wire? Hard steel is paramagnetic and so no such results (13421).

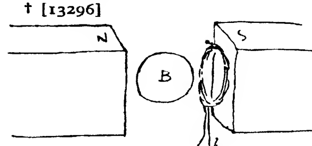
13299. Again, may not hard steel be forcibly employed to prove what I have said of the *nature* and *character* and *use* of the media surrounding a magnet (Exp. Res., 3278, etc.)?

13300. If a magnet, supersaturated, when cast upon its own resources in the free air, falls in power (Exp. Res., 3285) because

* [13295]



† [13296]



the air as a medium cannot sustain it–it would fall still lower in bismuth, it being a worse conductor–and in hard steel it ought to fall lower still, having its power crushed out of it and extinguished. So a fine needle, well magnetized, if placed between hard steel plates, ought to be greatly reduced in power; and if placed in a groove in one plate, just fitting it, and then covered by another, ought to be demagnetized. Good experiment.

13301. Numerous fine proofs of magnetical principles may be obtained by the use of well hardened perfect steel in magnetic fields not too strong in power, i.e. not so strong as to subvert the coercitive force of the steel itself (13421).

13302. Possible titles or heads to consider or determine separately:

Magnetic polarity.
Lines of magnetic force.
Magnetic media.
Magnetic conduction.

13303. Put the proofs together that hard steel is the extreme diamagnetic body (13421).

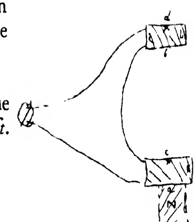
13304. Point out emphatically the value and power of the moving wire as a true natural means of investigating magnetic forces.

13305. Point out emphatically the principles of polarity.

13306. Read, take up and refer to (Exp. Researches, 3155, etc. 3277, 8, 9).

13307. *Pieces of steel.* Must look for the best steel to harden–and the best process of hardening. Must heat them in leather or cyanide of potassium, so as to have no iron or soft part on the surface–or else must remove the outer surface by cold friction on a sandstone. Must carefully avoid any decarbonized surface on the steel.

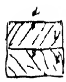
13308. A magnet reversed is in the same relation to zero as hard steel, but is as far beyond hard steel as hard steel is beyond zero. So the order would be: a hard magnet direct–bismuth or phosphorus–Space or Glass–hard steel–a hard magnet reversed. Iron is not in a fixed or nearly fixed state. As a conductor it is the reverse of hard steel (13421).

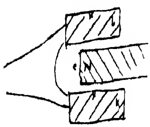


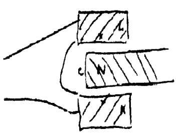
13309. Experiments with the apparatus (13223) continued. The N pole *to* either *a* or *b* sent the galvanometer needle to the *left*.

The pole N *removed* made the needle go to the *right.* So the helices are conformable and their currents also with like poles in like positions.

13310. The S pole applied either to *c* or *d* sent the needle to the left–when *removed* the needle went to the *right.* So the action of the N and S poles accords perfectly–and the effect is the same whichever magnet is employed.

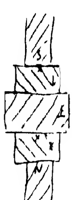




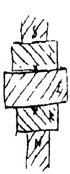


13311. K and L were put together as one helix, etc. The same results occurred when the poles were applied at *a* or *d* as before. Now K and L were opened about 1 inch apart and then the N pole of one of the magnets *put in* between them at *e* so as to act on both. The needle went a little to the *right*–when the magnet was *removed* the needle went a little to the *left.* This was the excess of action of K over L–for when L was removed the same application of the N pole to K gave a much stronger deflection of the needle in the same direction, and when L was turned round 180° and replaced, the N pole caused a very strong action, sending the needle to the left, for now both helices accord in their relation to the single pole N.

13312. The cylinder of soft iron E, being introduced at *e* in either of the two last positions (13311), did nothing. Nothing was expected, for no source of magnetism was then present.

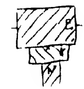


13313. Now introduced the magnets and also the soft iron E, having as yet no commutator but direct and constant direction with the galvanometer as above (13309); and beginning with E away, I expected that putting it into its place would be equivalent to an approach of either or both magnets. It was so. The needle went strongly to the *left* when the iron was put *in*–and as strongly to the *right* when it was removed.

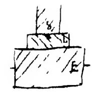


13314. Turned L round 180° so as to give it the anomalous position in respect of K–introduced the iron E, expecting only the differences of K and L. It was so. The iron *in*: the needle went a little to the *left*; when taken out, the needle went a little to the *right.* When L was away altogether and K left alone with the magnetic poles, then *introducing* the iron E sent the needle much to the *left*, and taking it *out* as much to the *right.* When L was there alone, then the iron put *in* sent the needle to the *right*, and when taken out it sent it to the left. All is in accordance.

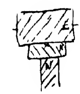
13315. The commutator was now introduced into the circuit but with the parts G and I so adjusted and left as to give like deflections as before, and for the first two following experiments it did not move. Then in order to obtain the true relation of *introducing* E and of *revolving* or moving E on its axis, the following expts. were made. *Withdrawing* N (which is the same as withdrawing E (13314)), the needle went strongly to the *right*—*advancing* N, the needle went strongly to the *left*, and that in whatever position E was placed, being moved round its axis and then left still.



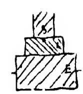
13316. In like manner, when S and L were tried, *withdrawal* of S made the needle go to the *right*—and its *approach* made the needle go to the left—whatever position E was in, provided it were still. All these effects of withdrawal or approach are accordant.



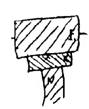
13317. Now N and K alone were reexamined as to the power they acquired by movement to and fro of E (13315) in its partial rotation, the part G of the commutator remaining still unchanged. The clam and connexions were so adjusted that just after the quick change of the commutator the *withdrawal* of N sent the needle strongly to the *right* as above (13315): before the quick change of the commutator, withdrawal of N of course sent the needle to the *left*. In this state of things, 20 rotations of the handle L (13223) sent the needle fairly to the *left* over and over again. So vibration of E is *equivalent* to advancing N (13315).



13318. Employed S and L in the conformable position. The withdrawal of S sends the needle *right* as before (13316). But 20 revolutions of the handle sent the needle feebly to the *left* again and again. So here vibration of E is *opposed* to the advancing of S.



13319*. The handle O of the experimental cylinder of iron has thus far been upwards, as in the figure. If carried downwards and then connected as before (13223), it will make a difference in the direction of the motion of the iron, for if the handle being above moves to the right, the iron will revolve in the direction of the hands of a watch, but if the handle being below move to the right, the motion of the iron will be the reverse of the former. Therefore arranged so that the handle should be *below*, and now resuming N and K (13317), 20 revolutions of the clam handle



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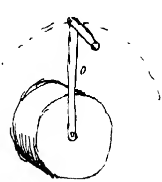
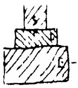




sent the needle well to the *right*, i.e. in the contrary direction to the course taken when the handle O was above.

13320. So also with S and L–20 movement[s] with handle O downwards sent the needle to the left.

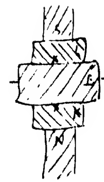
13321. So here is a step to the solution of the mystery why the helices K, L, must be in unconformable positions. When placed conformably, each can deflect the needle to the *right* or to the *left*, according as the handle is above or below, but that position of the handle which gives *right* deflection for the one gives *left* deflection for the other; and therefore, to sum up the two actions, one of the helices must be reversed or the two be in the *anomalous position* in relation to each other.

13322. Still the point to be explained remains behind: one would have thought that the two helices, etc. would when *accordant in position* have risen and fallen together in magnetic force by movement of the iron–but they do not. One rises as the other falls and vice versa.

13323. The length of the arm O has been such thus far as to give a vibration of E, the iron cylinder, through only 102° or 103°. So I have had it removed nearer to the axis, so as to give an Arc of vibration of 150° to the iron E. Repeated the former experiments (13317, 8) with this handle up and down, and had exactly the same results as before in regard to direction, but stronger, i.e. larger amounts of deflection.

13324. This shorter arm and increased arc of vibration of E gave me a better effect far than before, and I think I also saw the effect of *time*. I could easily now obtain 7° or 8° of needle deflection by 14 or 15 revolutions of the clam handle. Even 5 revolutions of the clam gave a good deflection. Two revolutions produced a sensible effect with N and K. Even one revolution of the clam sensible in its effect, and I think I could perceive the effect of *each* passage of the commutator in one revolution of the clam, both the quick and the slower one, or at least the effect occurring between these passages. So this large arc of vibration very valuable and shall easily obtain a good swing now. I could not move the handle so quickly as before, but I thought that the quickest motion was not so good as a slower one. If I mistake not, 10 quick revolutions or movements did not produce so good an

effect as 10 slower revolutions, both being well within the time of a swing of the needle. Must look at this point of *time*.



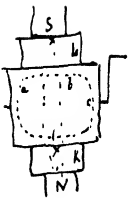
13325. Now placed all in association, but in the anomalous position, so as to obtain the greatest effect by movement. Ten revolutions of the clam handle gave now 10° or 11° deflection of the Galvanometer by vibration of the iron E, with handle either above or below.

13326. But if the iron E went round continuously either in one direction or the other, there was no permanent effect. The cylinder E assumed a condition which was no doubt stable whilst the rotation continued and therefore produced no current. It is evidently the alternate condition of the alternating iron which produces the effect gathered up by the commutator, and it is the real cause of this alternate condition and why the helices K and L must be in the anomalous position, that I want clearly to understand.

13327. *Clam action.* To verify the former results of clam position, I now varied its position so as to make the quick passage at the commutator occur 5° before and also 5° after the moment of rest of the experimental iron cylinder (13230), and I tried the effect produced when the handle was above (13319) and also when it was below. No other difference was introduced than that of *left* and *right* for the two positions of the handle. The clam was equally effective for both position[s] and its main effect was when the quick passage was at the moment of stop of the vibrating exp. cylinder (13233).

13328. Thought that the effect of the vibrating iron might be due to its intersection of the lines of force of the earth passing through it, and if so conceived that, if I put an S pole over the iron, it would counteract the earth's lines of force and send its own lines of force in a contrary direction through the iron. The handle O was below and the movement of the cylinder sent the needle to the *right*. But when I changed the upper S pole for an N pole, still the needle went to the right, though not so strongly as before. I do not think the effect is due to the intersection of the earth's lines of force.

13329. It is more probable that the effect is due to the intersection of the lines of force proceeding reciprocally through and between



N, K, E, L, S, all of which are magnetic and permeated by these lines. It is evident that these lines of force, being horizontal or nearly so, as the piece revolves continuously in the *direction of the hands* of a watch and in a vertical plane, the lines of force will be so cut or intersected as to form horizontal currents in E, which will go from *a* by *b* to *c* and produce an N pole below and an S pole above; whereas if it revolves against the hands of a watch, the currents will be in the contrary direction and therefore the N pole above and the S pole below. Every time therefore that the iron E vibrates, it will be equivalent to a vertical magnet, having poles in one direction as the motion is with the hands of a watch and in the other direction as it is against the hands of the watch.

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13330. The earth's magnetic force supplies a fine magnetic field for the steel experiments. Remember the dip (13421).

13331. Is there a nickel or cobalt steel? What would a tartrate of nickel or cobalt reduced by heat give?

13332. Cast iron. What is its place–hard, brittle–and also grey, soft?

13333. Examine compounds of iron in respect of magnetic force. Perhaps some not merely neutral, but having a relation to steel. Sulphuret, Phosphuret, Arseniuret, Alloy with Antimony, Hæmatite. Examine them in a feeble magnetic feeble [field] with a feeble needle.

13334. Employ Nitric acid or dil. S.A. to clear off the decarbonized or soft outside of hardened steel.

13335. Helix with E. current inside or outside of a soft iron cylinder. Reason of the difference in effect–appears to be very simple, if iron be considered as a magnetic medium. If the iron inside the helix, there is all the space around the helix to carry on the parts of the closed lines of force external to the helix. If the iron be outside the helix, only those lines of force can be excited in it which can be continued through the air in the helix and those are very few. Put iron there too and then the outside iron rises up. We obtain in fact Nickels[1] peculiar magnets. Put

[1] Presumably Jérôme Nicklès (1820–1869).

steel inside the helix and if it does not charge it should diminish the force of the outside iron.

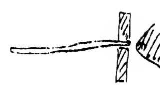
13336. Hard steel core in a helix having a moderate current of Electricity. Will it resist charge? If so, will a hard steel cylinder in a helix protect a soft iron core within it from charge?

13337. Can a hard steel cylinder cut off the power of a current in a wire, so that it shall cease to deflect a magnetic needle?

13338. Send a current of Electy. through a hard steel wire. The lines of magnetic force will be resisted as to their formation. Will the *conducting power* of the wire be varied thereby?

13339. A wire or flat helix carrying a current of electricity. Inclose it between two hard steel plates. Is the current affected thereby, either momentarily or permanently? Inclose it in iron plates. Is the current then affected?

13340. Length of conduction through soft iron wire about 0·1 of inch thick, protecting the origin of the wire by a thick steel plate with a hole in it. Examine the course and end of the wire by a small magnetic needle.



13341. Magnetic *lines of force* convey a far better and purer idea than the phrase magnetic current or magnetic flood: it avoids the assumption of a current or of two currents and also of fluids or a fluid, yet conveys a full and useful pictorial idea to the mind.

13342. Universe Magnetism. Earth, Sun, Moon probably all lie as mutually related magnets in the common medium of space. It is only on the supposition that one is reversed in its polarity to the other that we can suppose it insulated and existing as an independant and separate system of forces.

13343. In the view of Media, may very well speak of atmospheric magnetism in relation to the earth.

13344. The terms Paramagnetic and diamagnetic have perhaps now done their duty and but for convenience sake for a time might be allowed to pass away.

13345. Now the Sphondyloid of power will come into service.

13346. Supposed order of consideration?

Magnetic conduction—Conductors, etc.

True condition of a magnet and its surrounding media.

Magnetic polarity, as to bismuth, etc. etc.

13347. Space in a hard steel chamber is deprived of magnetic force, the force is shut out of it. This space or state of space is *new* to our knowledge? So also is the space filled with lines of force *new* to our knowledge, i.e. to the knowledge of philosophers generally.

13348. Though Iron and bismuth are antagonistic in air (Tyndall, etc.), they are not so in bodies more diamagnetic than bismuth or more paramagnetic than iron. Steel is more diamagnetic than bismuth perhaps and one might do something with a steel chamber.

13349. The space in a steel chamber occupied by iron or sul. iron – then it would be diamagnetic compared to other bodies in air. It and the chamber together would be sent outwards (13421, etc.).

13350. Iron without lines of force through it and iron with lines of force through it are diamagnetic and the other paramagnetic.

13351. The putting a steel chamber over a piece of bismuth or sul. iron or perhaps iron is the *shutting out* of the flood of power and sending it elsewhere. The state of the body as compared to its state when lines of force pass across it.

13352. Compare iron and bismuth in a steel chamber–one not paramagnetic and the other diamagnetic there.

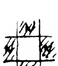
13353. The great philosophical value of steel in magnetism much [illegible][1].

13354. The great philosophical value of a steel chamber (13421).

13355. Revolving rectangle or circle in a steel chamber.

13356. The power and uses of the surrounding media and its necessity for the transference and even existence of the magnetic force gives the true explanation of the peripheric action of iron, pointed out by Barlow and others. A large iron core in a helix has only the external parts excited–they are the parts next to the source of curves, i.e. the wire convolutions, and they are sufficient for the curves due to a certain degree of current.

13357. Consider here the physical condition of the iron as a conductor. Shall get the key to the relation of conduction polarity and permanent polarity perhaps.

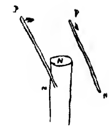


13358. The inside of a steel chamber (13421) coincides with the chamber enclosed by six square like poles, which must also be a place deprived of magnetism. There may be a tension in the

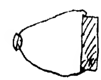
[1] Perhaps "increased."

latter (or there may not); there will not be in the former. Both are analogous to the state of the inside of a metal vessel charged with static electricity.

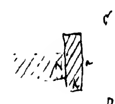
13359. The current produced in a moving wire as it passes from left to right over a North magnetic pole is from P to N in the wire. The current which will produce a magnet such as that represented has also the relation of magnetism and current shewn by the S and the arrows*.



13360. The helix K and galvanometer connected directly. When an N pole was carried from the outside to or towards *a* in any direction according to the arrows, such an approximation *to the center* of that side sent the needle to the *left*. When taken from that part outwards in any direction the needle went to the right. The S pole produced actions exactly the reverse. See Exp. Researches, 112, etc.



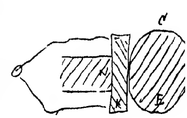
13361. A pole brought only to C or D did not produce a sensible effect, or only a very small one. But when the pole N was put up, then another pole N brought up to either C or D or *a* sent the needle to the *left*, and when taken away the needle went to the right. Just as before in direction when the magnet was away—and if a S pole were at N instead of an N pole, the effect of the N pole on the other side should be still the same (13411).



13362. When with the N pole behind (13361), the S pole was brought to C, D or *a*, the needle went right—and when taken away the needle went left.

13363. If an N pole is brought to C and an S pole to D, they should jointly neutralize each other and produce no current (13409).

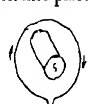
13364. Placed the Experimental iron E up against the helix and then applied the N pole at C and D; the effects were alike, and the needle went to the *left* on *approach* of N as before (13361) when iron away.



13365. When the N pole, being at C (or any of the other positions), is changed for an S pole, then the needle goes strongly to the *right*, and when that is changed for an N pole it goes as strongly to the left. This change contains I think the key to the phenomena (   ).

13366. Put both helices, etc. into place in the anomalous position,

* [13359]



also the magnetic poles N and S, and connected all with the commutator and galvanometer*. Only E was a box wood cylinder, but on it was applied a thin bent magnet, so as to make E north and south on opposite sides.

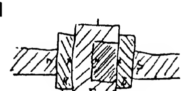
13367. When the *commutator was fixed*, and served only as a simple communication, turning E round continually did nothing. Turning E round 360° in either direction produced no final current. When one pole on E was above and the other below, turning E 180° so as to change the pole places produced a current deflecting the needle. Turning it another 180° so as to restore the cylinder E to its first position produced an equal contrary current.

13368. As the N pole goes from above and the S pole from below, the needle moves to the *right*. As the N pole goes from below and the S pole from above, the needle moves to the left. It does not matter which way E revolves, whether with or against the hands of a watch, the effect and direction is *the same*. So it is the inversion of the polarity of the cylinder E that produces the effect. This looks like the result of induction current for the iron cylinder E (13329).

13369. Moving the clam handle by hand once round for one revolution of E, I had little or no effect at the galvanometer whether the quick return of the commutator was just before or just after the inversion of the E magnet. But as this clam acts twice in a revolution, so it gathers up counteracting currents and finally produces little or nothing. Therefore worked the clam handle through 180° whilst the cylinder E revolved in two movements through 360°, and now had strong swings to the *left* and to the *right*, just as I happened to begin, with the clam handle in a given direction or 90° forward, which of course changed the order of direction at the commutator.

13370. So I replaced the clam by a single one, i.e. by one which let the commutator piece I (13223) pass quickly one way and then slowly back again in one revolution of its handle. The number of its periods now coincided with those of the Exp. cylinder E. And now I obtained large deflections of the needle when both handles revolved through 360° in the same time. For revolution of E through 180° gives a current, and then its passage through 180° more gives a contrary current, and these the commutator

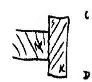
* [13366]



can now properly gather up when its changes are made to correspond with the upper and lower positions of the magnetic poles of the revolving E cylinder.

13371. Dismissed the E magnet (13366), and returned with this knowledge and the single clam to the soft iron E cylinder (13364). Connected the clam handle to the handle of E as before by a rod (13323), the clam being so that the quick change (and of course the corresponding change back) occurred when the cylinder E came to its momentary rest. Now four revolutions of the clam handle gave an excellent deflection of the galvanometer needle. But whether the handle of E was below or above, i.e. whether E vibrated with or against the hands of a watch, the deflection remained the same, the needle going to the right; though the polarity of E from induced currents must have been reversed.

13372. Now whether the N pole moves to right or left makes no difference with N and K (13361), but its changing here from above to below made a great difference. Here the poles do not *move* from above to below, but they keep their places, *changing* however from N to S and vice versa. But the recession of the N pole from C is the same thing as the replacement of it by an S pole, only the latter change has double the power of the former. Also the approach of an N pole to C is the same thing as the change of an S for an N pole there. Also the approach of an N pole to C (which is the same in effect as the approach of an N pole to D) is the same thing as a change from S to N at C; whilst the change simultaneously from N to S at D is equivalent to the approach of an S pole at D. But that would seem to say that a change equivalent to an approach of an N pole at C, and another equivalent to the approach of an S pole at D, conspired to produce the same kind of effect. Whereas N and S poles in these places neutralized each other (13363, 72). Is the resultant an oblique action changing from side to side, and is that the cause of a result?



13373. Changed over the piece G of the Commutator (13223) so as to have the time of the bearings of the connecting springs more equally divided.

Six revolutions of the clam, etc. with handle of E below gave good deflection to *left*.

Do. . . . . above gave very good deflection to *left*.

13374. Altered the clam so that the change at the commutator was in the middle of the 180° motion of E instead of at the end, and obtained little or no effect. Returned the clam to its effectual position, that the commutator might change at the moments of rest of E as before.

13375. The clam handle and the E handle, being connected by the rod M, and the shorter arm of E used, it was easy to make both revolve and E in either direction only if the clam handle went with an equable motion; it is evident that E in going round would make two momentary stops 180° apart, coinciding with the passages at the commutator, and would then gradually gain and lose velocity, the quickest motions being 90° from the former. Now carried clam handle round equably, and the cylinder E round continuously in one direction, as the hands of a watch; the Galvr. needle went strongly to the left. This occurred again and again. Did the same thing, except that I made the revolution of E continuous in the contrary direction and against the hands of a watch. The Needle still went left.

13376. No matter which way E *revolved*, whether to the *left* or to the *right*, or whether it *vibrated* with the handle *above* or *below*, the needle constantly went to the left.

13377. Separating the cylinder E from the clam handle and leaving the latter quiescent, rotation or motion of E did nothing. Of course motion of the clam and commutator without motion of E did nothing. The effect is clearly due to their joint action, and I believe to the intermitting motion of E.

13378. So moved E as regularly as I could with one hand and the clam to correspond with the other, and then I obtained little or no action. With a perfectly regular motion of E, there would be *none*.

13379. The connexion of the clam handle and the cylinder E handle by a rod M (13232[1]) necessarily causes that when the one handle is to the right the other is also, and the same coincidence occurs on the other side: all this, whichever way E revolved. So took off the rod M, and moved both handles by hand: then the hands could be both moved to the left or to the right at once, representing the rod action, or approach to and recede from each

[1] ? Par. 13223.

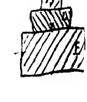
other. The first kind of motion gave the same kind of effect as with the rod–the needle went to the *left*. But the second kind of motion gave an equally strong *reverse* effect, the needle going to the right, and that in any direction of motion of E.

13380. This change in the hands only operates in reversing the contacts at the commutator, and that can be done as well by turning the clam 180°. Did this, and then using the rod connection as before (   ), the deflection of the needle was reversed and became the same as when the hands moved to and from each other. The clam altered 90° from this position gave a poor effect as before (13374).

13381. The currents must be due to the intermittances in the velocity of E rotation or vibration. Now as the cylinder goes round in one direction, a pole, say N, will be generated and fade away above in every half revolution, and an S pole at the same times below. The point is, why do not these poles neutralize each other in their action on K (13384)? As the cylinder revolves in the other direction, the N pole will be below and the S pole above. As to likeness of action, the N pole whether below or above has like action (13361) if it be alone, i.e. not associated with the S pole; but what is the resultant action of the two poles N and S of the E cylinder?

13382. Between one passage of the commutator and the next, the movement of the cylinder E comes on, is at a maximum and then falls to nothing. Then the commutator changes and gathers up a second current, which must by the effect be in the contrary direction. But if the cylinder is continually revolving in one direction, how can the current be changed, for the polarity induced in E must be the same as before? Trace this out in presence of the apparatus.

13383. The two helices and their magnets were tried alone, i.e. N, K, with E and S, L, with E, the anomalous position as the most effective being preserved. They gave like results, for revolution of E either way gave *left* hand deflection of the needle with both or either one of them.



13384. If the resultants of the magnetic forces of the fixed magnets and the poles formed in the moving cylinder E be considered

and the position be as sketched*, E revolving in the direction of the hands of a watch, then whilst moving, an S′ pole will be formed above and an N′ pole below. These poles and the old ones would influence each other; S would be weakened in the direction towards *a* and strengthened in the direction towards *b*, whilst N would be weakened towards *c* and strengthened towards *d*, and an oblique resultant of action would result, *e*, *f*. If E were revolved the contrary way, then N′ would be above and S′ below, and an oblique result. of action would result, *ė*, *ḟ*; but *e* or *ė* would always be on the side of N, and *f* or its like *ḟ* on the side of S, and there is reason to believe that being above or below would make no difference (13361). The point is, that replacing the cylinder E by magnetic poles, will contrary poles at *e* and *f* produce conjoint actions in the anomalous helices; and why is it that the commutator changes at the *beginning* and *end* of their existence and not in the middle, when it may be conceived that the chief *change* (from N to S) occurs?

13385. Will like effects occur when E is made of copper? Wait for its results.

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13386. *Title* for first paper—perhaps—or at least the subject: *On Magnetic conduction and magnetic conductors* (13421).

13387. I suppose all diamagnetic bodies, as wax and water, resist the induction just as paramagnetic bodies do—just as steel does for instance, and iron, but so much less. Then should require to know the degree in which they give way and the time when it comes on—I mean of that induction which lets the body return to its first state when it is removed, not of that which is permanent or which gives *magnetic set*.

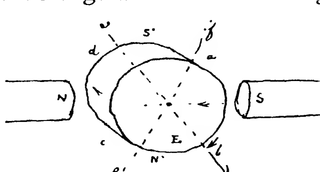
13388. Should also require to know the resistance to magnetic set. What is that resistance for steel (13421, etc.)?

13389. Have all bodies a condition equivalent to magnetic set? If so, how is it shewn and when does it come on?

13390. Table of order of *magnetic conducting power*.

13391. Table of the order of *conduction polarities*. The order will be just as the media, one above another. The polarity (conduction) of a substance will change with the surrounding medium, being

* [13384]



paramagnetic in a diamagnetic medium and diamagnetic in a paramagnetic medium. But this not the *initial polarity*.

13392. Perhaps steel, even when just hardened, has that amount of magnetic condition which is equivalent to the earth's force in the space occupied by it when hot. Then it ought to set to a magnet carefully adjusted and not too strong. If it has, it must be hardened in a *Magnetic vacuum*, as in a steel or an iron box (13421).

13393. Hard steel may appear magnetic by its action on a needle—just because it is *not* magnetic, disturbing then the order of the lines of force.

13394. Perhaps some valuable media found amongst the alloys of steel and Iron. Zinc Iron alloy: what is its quality as a medium?

13395. The Ring magnet is a beautiful case of a magnet needing no surrounding medium. The reason why? The lines of magnetic force being closed curves are all complete within it.

13396. Yet they have *some external force* or some kind of external force; for when a double helix is round it, a current sent through one helix induces a current in the other, which is greatly increased by the soft iron acting as core.

13397. We live and experiment within a magnetic flood of force and subject to a mixed magnetic medium. We ought to live and experiment in a hard steel or an iron house. A good soft iron chamber will give a magnetic vacuum when it is wanted.

13398. Hard steel ought to be like bismuth but *better*, in Weber's experiments and mine and the rest about diamagnetic polarity.

13399. Steel is more opposed to iron than bismuth is. Yet steel is not reversely polar; therefore bismuth is not. In any case, the polarity of bismuth is the same in kind as that of steel and not the *reverse* of iron.

13400. *Initial polarity*. Perhaps accept that as polarity which belongs to an independant permanently excited body, as a steel magnet, a loadstone, a helix with its electric current. How is the polarity of the soft iron ring magnet related, and how are all these polarities related, to the ring of polarity round an electric current?

13401. *Conduction polarity* is variable for the *same* body, *because* it depends, not on the body itself only, but upon the surrounding

media. The initial polarity does not depend on the medium in the same manner, though it does in most cases, for its existence.

13402. Hard steel polarized, i.e. magnetized in one direction, would have all the characters of a crystal of bismuth.

13403. Iron struck in one direction in a soft iron chamber: does it then resemble a bismuth crystal?

13404. Much doubt of the physical notions of magnetism–currents or fluids. Would be better to search for some new idea of it.

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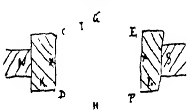
13405*. Connected the helices K, L, the commutator and the Galvanometer together. The Galvanometer was fixed, the magnets N and S in place and the helices conformable.

13406. An N pole *to* C, D or A sent the needle to the *right*. An N pole to E, F or G sent the needle to the left, or the contrary of the former. An S pole produces the reverse actions.

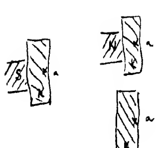
13407. An N pole from A to G sends the needle strongly to the *left*: when passed from G to A–it goes very strongly to the right. An N pole at A is equivalent to the neutralization or removal of the N power there, but at G it is equivalent to an addition to or doubling of the power of the S pole there.

13408. The equal opposite poles of my good Logeman Magnet (horse shoe) applied at C, D, did *nothing* (13363, 72). Removing them did nothing. The same was the case at E and F. Yet this must have deflected the power of the N and S poles at the helices very much indeed. So opposite poles at C and D would, and did, do nothing.



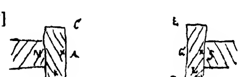
13409. Made the helices anomalous. Then opposite poles applied simultaneously at C, D, or E, F, or G, H–did nothing.

13410. An N pole at C sent needle to the *right*–the same pole at E also sent the needle to the right–as was to be expected in this *anomalous* position. An N pole moved from C to E did nothing but what depended upon the superior power of helix K. When moved only from E to I and back again properly it did *nothing*–all as was expected.



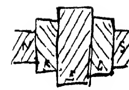
13411. Put helix K with an N and an S pole and also without any magnet. Then an N pole up to *a* sent the needle to the *right* in all the cases–and when taken away the needle went to the left.

* [13405]



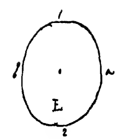
The N pole which is brought up does its own work under all the circumstances; disturbing the forces in the helix in the same direction.

13412. The magnets differ in strength as well as the helices. Have now associated the stronger magnet with the weaker helix L: both helices being in the anomalous condition or position. The iron cylinder E in place and the commutator with the single clam (13370). The rod M (13223) connected the clam handle with the short arm of cylinder E.



13413. All worked well. Six revolutions of the clam, i.e. six *to* and *fro* of the handle of E gave a good swing to the right.

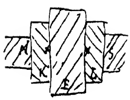
13414. Then as to the simultaneous action of the cylinder E and the commutator under these circumstances. The commutator conducts for equal times in contrary direction, and these times are just the times during which E is revolving or vibrating between stop and stop. If the cylinder E vibrates, the current is all one way as the cylinder moves in one direction and all in the other as it moves back again, i.e. as the part *a* moves to *b* by 2 or below, the current is *one way*; and as it moves back by 2 or below, it is the *other way*.



13415. But if E revolve instead of vibrate, the same effect occurs; i.e. if the part at *b* goes back to *a* by 2 below, or forwd. to *a* by 1 above, the effect is the same for 6 revolutions of the clam handle. But then, what is the difference in the condition of the iron cylinder which causes this reverse current? The return of the part at *b* by 1 to *a* is exactly the same thing as the progress of *a* by 2 to *b*—yet the one gives one current and the other gives a *contrary* current. What makes these two halves of revolution of E in the same direction give contrary results? I suspect the cylinder is magnetic.

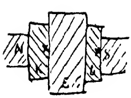
13416. So revolved the clam and with it E as the watch hands six times: it gave a good result, needle to the *right*. Turned the iron E on its axis thrgh. 180°, and now six revolutions gave the needle to the *left*. Turned E on its axis until I found a position in which six revolutions as before would do *nothing*—and when turned 180° from this position it also did nothing—but when turned 90° from either of these positions, then it was as strong as ever.

13417. Marked the part of the cylinder which was upward when the cylinder came to a stop in its motion; then took it out and examined it carefully by a small magnetic needle—and at last found a northness near this place and a Southness on the part diametrically opposite to it. So here is the explanation of the whole. The iron is magnetic and acted just as the little magnet attached to the wooden cylinder E (13367, etc.)



13418. So removed the iron E and put in the bismuth E, with both helices *anomalous* at present. The single clam was in and its handle connected with the arm of E by the rod M (13223). The clam made the passes when the cylinder E stopped in its rotation. Now whether vibrating *below* or *above*, or revolving in either direction, still there was no current—no motion at the galvanometer.

13419. Put on the double clam, making the commutator changes to occur at the beginning and middle of each of the 180° journey of the bismuth E. Whether vibrating below—or above—or revolving with the clock hands—there was still nothing at the Galvanometer.



13420. Put L into the conformable position. Now when the handle of E was above, there was a minute trace of current to the *left*. When the handle of E was *below*, there was a trace of current to the *right*. This effect, though doubtful, is worth observing with the Logeman Magnet (13466).

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13421. Now then for all the foregoing fancies about Steel, and to ascertain if there be any thing in them; it is perhaps not to be expected except in a field of very weak magnetic force, for strong force of course makes it like iron in most points. However, try.

13422. I have had some steel buttons hardened, as much as possible by me. I have made some small magnetic needles out of pieces of a sewing needle, one sixth of an inch long, which are suspended by 6 or 7 inches of cocoon silk. The buttons are free from initial polarity.

13423. Any part of the edge of one of the buttons could attract either pole of the little needle if sufficiently near. So the hard steel cannot resist the inducing power of the little magnet, but

acquires for the time conduction polarity. Lines of force fall on to it and not only pass through it but converge upon it. Will a weak needle do the same thing? I expect it will—try one, of a piece of weakly charged loadstone (13468).

13424. Then as to the effect on the hard steel of the Earth's magnetism. It not only passes through the steel (hard) but converges upon it, giving paramagnetic polarity (conduction). For considering the direction of the terrestrial lines of magnetic force, the top of the button held in a vertical plane attracted the N end of the magnetic needle and repelled the S end; whilst the bottom of the button produced the contrary effects, and that whatever part of the edge of the button was uppermost. So the Earth's force makes the button polar for the time, just as it would iron, and the steel loses the state instantly by change of position, not having acquired initial polarity. So there is very little hopes of steel for any effects of a diamagnetic character; but must try again when the outer coating of oxide and iron has been removed by acid or grinding.

13425. I made the earth's lines of force weaker by the use of a counteracting magnet, but the results were the same—and accordant even through the condition and into the state of inversion of the lines of force. The hard steel appeared always to be paramagnetic.

13426. Hard steel appears to be always paramagnetic even in the feeblest magnetic field; but as compared to iron, it soon seems to take magnetic set and then strongly to hold the set. It certainly cannot conduct as well as iron, even when it has the advantage of an amount of set which it cannot lose again for the time; and that is easily shewn by soft iron and hard steel cores in a helix carrying an electric current.

13427. The hard steel conducts a certain amount of force easily without magnetic set, and as easily loses it again. But iron conducts far more easily. It cannot conduct a greater amount without acquiring *set*, and *then it remains a magnet* after the inducing power is withdrawn. In this respect iron far excells it, for it can conduct a far greater amount without *acquiring set*—and can return as the induction falls.

13428. So hard steel *resists* the assumption of the *conducting state* and yet *sets soon* under it—then *resisting* the change back again.

13429. But on bringing hard steel and soft iron towards a magnet, so near that the steel has taken some *magnetic set*: the steel with set and the iron without set must I think be in the *same state* whilst they remain in *like approached conditions*, i.e. those which brought on the induction; though the one retains much and the other very little of these states when they are removed. Hence the state retained by steel would seem to be that belonging also to the iron whilst it was conducting.

13430. So the term polarity may apply for the time to both, but I think it must be ever dependant upon the direction of the lines of force in those bodies. It should not merely imply a concentration of the lines of force, as in a magnet or a conducting body, but also their direction. Else polarity might change whilst direction of the lines of force would remain the same, as weak sol. Sul. iron in stronger solution or in water.

13431. If bismuth have the contrary polarity to iron, it should have like polarity to a reversed magnet. But a reversed magnet by rotation in a magnetic field will give one current of Electricity, and the bismuth by rotation in the same field will give the contrary current.

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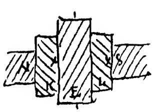


13432. Made E *copper*, being a metal that would give no doubt any effects due to induction of currents in it by motion. Had on the double clam with the quick passages of the commutator at the momentary stops of E (13230). K and L were placed in the conformable position.

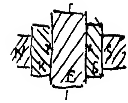
13433. The handle of E being above, 12 revolutions of the clam gave a trace of motion in the needle to the *right*–again also the same effect. Handle of E below–12 revolutions of the clam gave the same indication to the *right* feebly.

Suspected this arose from a little shifting of the magnets. So weighted them and held them in place. Then revolving E with the clock hands, or against clock hands, or oscillating it with the handle above or below–gave *nothing*.

13434. Put on the single clam (13370), making the commutator passages occur at the stops of E. But oscillation with the handle

below or above, or rotation with or against the clock hands, gave *nothing*.

13435. Turned the clam 90° so as to make the commutator passages occur in the middle of the motion of E. With the handle above or below, there was *nothing* sensible. When rotated with the clock, perhaps the needle went the smallest degree to the *left*—and when rotated against the clock hands, perhaps the smallest degree to the *right*; but I rather think it is nothing or a mere accident.

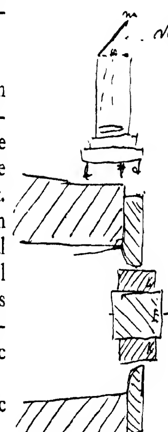


13436. Made K and L anomalous—the copper E still being in—and the clam (single) turned back through 90°, so that the commutator passages are at the stops of E. With the handle above or below, or E rotating with the watch hands or against it, still *nothing*.

13437. Advanced the clam 90° (13435). Neither rotation nor oscillation gave any result.

13438. The double clam was put in (13432), the quick commutator passages being made at the stops of E. Neither rotation or vibration did any thing. Altered the clam 45°—still nothing—nothing—nothing.

13439. So the currents induced in the copper do nothing. They must be there, but appear to be so perfectly balanced in their action as to produce no effects. Indeed I was not expecting any. But now if effect should appear with the Logeman and the Bismuth—*it will not be due to current.*



13440. I find the Logeman great magnet may be arranged with the apparatus in my room without disturbing hurtfully the Galvanometer. Also that by the use of the small observing telescope (   ) at the side of the Logeman, about 9 or 10 feet from the Galvanometer, I can observe the latter without going up to it. For which purpose I have put a mirror *m* at an angle of 45° on the flat glass top of the galvanometer, which sends the vertical ray from the dial in a horizontal direction to the telescope. All acts very well. Also have applied the two square terminations (   ) to the ends of the Logeman and to the helices as represented in the figure. All will do very well as far as magnetic force and arrangement are concerned.

13441. Revolution of a round hard steel magnet in a magnetic

field might give some very instructive results, as the field about it is stronger or weaker and as the sphondyloid[1] of power is compressed up into the round magnet or allowed to expand entirely into the surrounding space, i.e. the outer magnetic field.

13442. Compare such a magnet with a like piece of iron—of bismuth—of copper, etc.

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13443. Might write a useful paper at the present time—stating strongly and in the way of tests those points which may serve as settlers between my theory or hypothesis and the former ones—asking for answers upon the old theory and shewing the answers from the new one.

13444. The hypothesis is not so much mine as one renewed from old times. Look at Euler's letters and what he says.

13445. *Point one.* The supposed reverse polarization of bismuth and diamagnetics. Look at the atoms of bismuth. Either they are not polar to each other or they must be alternately the reverse of each other. If the first be true, then the whole cannot act as the whole of iron; the power of the magnet would not be to repel the mass but to act upon the atoms as couples and turn each round 90° in its place. If the second be true and they are polar to each other, then the sum of their polarities must leave the mass without any polarity at all. If it be supposed that the inducing poles act on the intermediate particles, passing by altogether the near particles, i.e. that the power acting on the third particle is not transmitted by the second, then that involves other odd and contradictory points and is not as the polarity of iron.

13446. Again. Thomson's perpetual motion.

13447. Again. Polarization of a medium solution in stronger and in weaker solutions.

13448. *Point* II. The moving wire results—and the polarity which it makes manifest whether in the diamagnetics themselves or amongst them.

13449. *Point* III. Curved lines of magnetic force, or else the independant sustentation of the P. and N. state when unrelated to each other, as in a straight or globular magnet. I say that the

[1] See *Exptl. Res. Electy.*, vol. III, p. 422, *footnote*, regarding Faraday's use of this word.

lines of force are continuous curves; they that they are cut off at the M. poles. May compare them perhaps to an insulated battery, where the internal power sustains the terminal state. But even there, the state not sustained unless the static induction allowed, and if that be prevented, the battery returns upon itself notwithstanding the tension. My view is that surrounding medium essential. So look for cases to prove it.

13450. *Point* IV. Impossibility of sustaining a long thin magnet equally charged from end to end, i.e. without consecutive poles, in a bad conducting medium as air. It is not sustained. Magnetise fine straight long wire by a helix in air, and examine it (13472).

13451. The iron wire ring in the helix gives the case of a very long magnet sustained because the curves re-enter. Coil a steel wire round a cylinder of glass, connecting its ends, and then magnetize it by a second helix round it. Examine its state when ends a little open and again when opened to full extent.

13452. Consider cases of very long and very short magnets as bearing on the question.

13453. Point V. Places of no force: either in iron chambers or between charged poles.

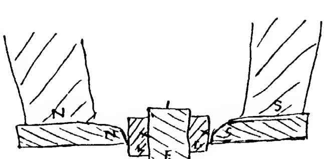
13454*. The great Logeman Magnet with two moveable terminals, associated with K and L in conformable positions—E is the *copper* cylinder—the double clam is on the axle, and the quick transition is 45° aside from the stop in the motion of E—it is as it was left on the 16th (13438).

13455. Rotation of E with clock hands sent needle a little to the right. Rotation in the reverse direction sent needle a little to the left as seen in the telescope (13440). When the needle goes to the left looking directly at the galvanometer—it moves to the right as seen in the telescope. Again obtained the same effect quite clearly. This cannot be due to any shaking of the poles together, for that would produce the same both ways.

13456. Oscillation to and fro—with the handle above or below made the needle move a very little to the left. Rotation to the right sent the needle to the right. Rotation to the left sent the needle to the left.

13457. *Removal* of either pole of the magnet sends the needle to the *left*.

* [13454]



13458. Set the double clam so that the quick commutator passage was at the stops of E. Revolution with the clock sent the needle to the right–revolution against the clock did nothing.

13459. Put the clam so that the commutator quick passage was in the middle of the motion of E. Rotation with the clock hands sent needle to the *left* hand–rotation against clock hands sent the needle to *right hand*. Rotation with the clock–*nothing*.

13460. Further rotations and vibrations, but very irregular and the effects always small.

13461. Put on the *single clam* (13370) with the quick commutator passage at the stop of the E motion. Rotation or oscillation gave *nothing*. Set the clam round 90°. Still no effects.

13462. Restored the *double clam* (13454), making the quick commutator passage at the stops of motion of E. Revolution with clock hands made needle go right as before (13455); revolution against the clock, needle to the left. Seems pretty constant when all in order.

13463. Vibration, whether with handle above or below, made the needle go to the right. Then revolution with clock–needle to the right; against the clock–needle a little to the left. With Vibration, with handle either above or below, needle always to the right.

13464. As the cylinder E was a little loose on the axle, I made every thing tight, cylinder, handle, etc. etc. Now obtained *nothing*, either by *rotation or vibration in any direction.* I suspect this is the true result with this commutator and that the effects are because of a little motion retained by E when it has been supposed to stop. Must have a commutator which brings up the motion of E suddenly, and *not gradually*, as the present one does.

13465. Reversed the helix L so as to make the two anomalous in position. Rotation with or against the clock–*nothing*. Vibration below–much to the right (accidental). Vibration above–a little to the *left*. Consider the results as *nil*.

13466. Put helix L conformable again and put the *bismuth* cylinder E into place. Whether rotated or vibrated–*nothing*. Turned L into anomalous position–still *nothing–nothing–nothing* (13420).

13467. If northness cannot exist in the presence of northness,

neither can it exist by itself or independant of the presence of Southness. It is a dual force and requires the influential presence of the S force. Both or neither must be there.

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13468. In reference to the Experiments with steels (13421), I have had some digested in dilute Sulc. acid to remove the outer coat of partially decarbonized iron, but they act as the former pieces did. I have also made a magnetic needle of a minute piece of week loadstone—it is able to be attracted and repelled by the steel and so shews that, weak as it is, it can induce the paramagnetic state in the hard steel.

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13469. Exp. Revolve two rings or rectangles in the chambers formed by Six like poles (i.e. tension chambers), one in each chamber, connecting them in the chambers with the magnets and externally with a connecting wire and galvanometer. Vary the connexions and rotations, etc.

13470. Place a wire across the tension chamber of *six* and also of *four* poles: rotate it when across the middle, or nearer to one side or angle: examine it by a Galvanometer connected with its ends.

13471. Perhaps some communications to the Phil. Mag. in aid of the development of the theory I hold (which is Euler's or older) of the following kind.

13472. 1. The opening out of the ends of a long wire magnet (13450), and the proofs derived from and connected with it, of the necessity of the surrounding medium.

13473. 2. *Polarity* (of conductors) not in the body only, but in it jointly with and dependant on the like condition of the surrounding medium. This is not the polarity of the magnet.

13474. 3. *Moving wire.* The condition of the space around a magnet is shewn not to be nil, by the state of the wire there. Its motion could not produce the relation between it and the magnet if the power were not there before and had hold of the wire. The common view will not account for the effect unless curved lines of force be allowed. Make the point of curved lines and that of

their physical nature distinct by reference to these and such considerations.

13475. The power of these lines is shewn by Elkington's machine, Ruhmkorf's apparatus, etc.

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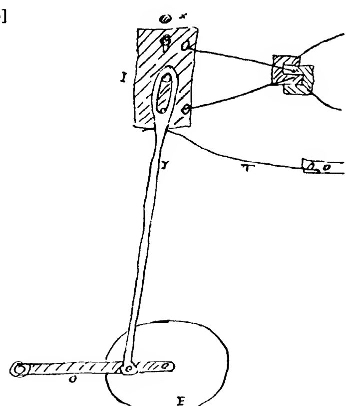
13476*. I have had a new commutator made, acting in a vertical plane. O is the lever or handle of the experimental cylinder E (13222[1]), and the parts of the commutator (13222[1]) are arranged over it on a vertical board, the spring T being below and keeping up the piece I against the pin X when at liberty. As therefore the lever O and exp. cylinder E is carried from the position figured round with the watch hands through 180°, the connecting piece Y rises, lets the piece I rise up at once against X, reversing the commutator from the position given, and there it remains until about 10° of motion only have to be completed, when the handle descending on the right side changes the commutator instantly and it remains changed until the motion back commences. So in describing 180°, which is done by hand, stops being applied on both sides to receive the handle O, the first 10° of slow motion sets the commutator up; it remains so whilst 160° of motion with accumulated velocity is on; the last 10° changes the commutator in the midst of this rapid motion, and then the motion suddenly ceases, that any effect produced by such cessation may be gathered up and carried on to the Galvanometer. The same happens on the opposite side, so that any currents due to displacement or

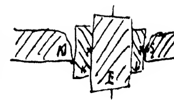
[1] ? Par. 13223.

* [13476]



accumulated motion, and any currents due to cessation of motion might be conjoined in their effect (13221).

13477. With the Logeman magnet (13454), the helices placed consistently, the bismuth E in place, and this new commutator, made experiments. The connexions were such that when the magnet pole was a little *withdrawn* the needle went to the *left*, and when a little *approached* the needle went to the right.



13478. When the bismuth was rotated, there was a little tendency of the needle to the *right*—it was very small—and as the approach and therefore strengthening of the magnet force (13477) made the needle go *right*, I thought it might be due to the taps on the blocks shaking the poles and causing them through attraction to set a little nearer to each other. The effect was very small.

13479. I then put in the copper E, and had a very small effect to the left.

13480. I took away the terminal pieces N, S, of the magnet (13454) and put in the annealed iron cylinder E; there was by motion a trace of action, the needle going left. This not consistent with a gradual strengthening of the action through the iron. Turned the iron round on its axis so as to have it in different position, and then found that sometimes the needle went a little left and sometimes a little right. So the result is probably due to a trace of magnetism (13417).

13481. As none of the peculiar effects expected come out by the use of the iron (13221), I conclude they are either insensible or nil; and therefore I conclude they cannot be expected with bismuth—and consequently *that line of research ceases here.*

27 OCTR. 1854.

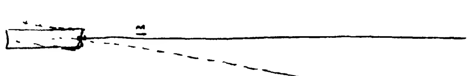
13482. Expts. on the magnetization of long wires of steel and iron, i.e. on the character and condition either of long magnets or of iron as a long narrow conductor.

13483. Soft annealed *Steel* wire $\frac{1}{70}$ of inch in thickness had a length of 8·5 feet stretched on a pole—being placed in a horizontal plane and perpendicular to the dip so as to be free from terrestrial induction, it was examined by a minute magnetic needle, about $\frac{1}{6}$ of an inch long, suspended by Cocoon silk. It had no magnetic charge but was alike every where.

13484. Two excellent bar magnets made of hard steel, being selected, were hence forwd. employed. They were 6 inches long; 1 inch wide and 0·25 of inch thick. One of them N was applied for 3 inches to one end of the wire and *tied on**, being left to induce and act on the wire simply by apposition. Using the small examining needle (13483) in the same position of the wire as before, there was no sign of any effect at the other end of the wire. On making the wire, rod and magnet travel end on so as to have the parts in succession brought within $\frac{1}{4}$ of an inch of the examining needle, there were no signs of any effect until, coming near the magnet N, the needle was influenced by it; and when from 9 to 6 inches or less off, set end on to it. But so little was the steel magnetized, that (looking at the plan†), when the rod and wire were moved round a vertical axis at N, so as to have the wire at M either under the test needle, or on one side or the other, the needle was not sensibly deflected from the position which the magnet N alone gave it, i.e. the vicinity or removal of the wire on either side made no sensible difference at distances from one to 9 inches or more between the magnet and M. I believe that the steel was magnetic in that neighbourhood, as the pole touching it, and was a sort of prolongation of that pole; but so feeble as to have no sensible influence in disturbing the needle whilst subject to the magnet pole N. At all events, the steel as *a whole* is not a magnet—the further end is not N and, even 2 or 3 feet off, a diffuse N does not appear.

* [13484]

† [13484]



13485*. Now the second magnet had its S pole placed in contact with the N pole of magnet N and also upon and against the steel wire, being held a little inclined, and was then drawn steadily and at one motion to the other end of the steel wire and tied on there, as the N magnet was, so that the steel wire remained as a magnetic link between the two poles†. On being examined by the needle, to see whether it was in a uniform or a gradually progressive state, having that or any state still sustained by the contact of the two chief poles, it was found that there were many consecutive poles alternating with one another all along the wire. So that this is not a means of magnetizing a long steel wire uniformly.

13486. The wire was not cleared from the coat of black oxide occasioned by annealing—nor was it stretched—so that it bent suddenly here and there according as the coat gave way in this or that place; hence it was not a straight line but irregular and the magnet S, in travelling, did not touch the steel itself except accidentally here and there. This state not good.

13487. Removed the magnets N and S and again examed. the wire, beginning at B and going on to the other end. B end was now left a north or N pole, as it ought to be; but the part within an inch of it was S—then came irregularities. About 15 inches on, a good N pole again and within half an inch of it, a strong S-ness on both sides—at 18 inches N again—at 19 inches S—at 21 inches N, etc. Then irregularity; N's and S's succeeded each other all along the wire till three inches from A, where the wire was strong S, and at the very extremity N. The terminal three inches at each end were not drawn over by the magnets and these three inches would simply serve as conductors of line[s] of force from the magnetic poles backwards towards the outer extremities.

13488. This long steel wire was then cleaned thoroughly by sand paper and $5\frac{1}{4}$ feet of it transferred to a shorter rod of wood—placed in position as before and examined (13483); it remained irregularly magnetized. One magnet was fastened on and the other drawn over it as before once, but now touching the metallic steel, and then fastened as before (13485). On examination, the steel was found irregularly magnetic as before, and the same was the case after the magnet was removed. So though the moving

* [13485]

† [13485]

magnet *touched* the steel, it had not obliterated the former state, or rather had not produced a *uniform* or *progressive state*.

13489. Began with a new piece of the same steel wire. Cleaned it well—stretched it until it broke and then extended $5\frac{1}{4}$ feet of it in this unexceptionable state on the shorter wooden rod. Examined it and found it in a good condition (13483).

13490. The N magnet attached to one end and the S magnet drawn once carefully along and made fast at the other end as before. Examined—there were numerous consecutive poles.

13491. Drew S magnet several times from the one end to the other. Still it always left numerous consecutive poles. So new clean steel wire and magnets will not supply a regular long magnet.

13492. Whilst the magnets were attached to the wire, after the drawing operations, the part of the wire near the magnetic poles had the same polarity with them; for repeating the experiment described (   ) and figured in plan*, the wire was found able to deflect the needle when on either side of it as the pole did; and when beneath the test needle it much increased the rapidity of its vibration[s] when near, beyond those produced when it was depressed, shewing lines of force thus disposed†.

13493. When the magnet N was taken away, then the part touching it was left in the contrary state, but the part an inch or two off was left as before, giving the first of the consecutive poles.

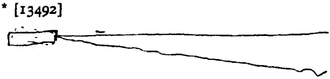
13494. This is a cause of complication in the final state which a helix perhaps may not give.

13495. *Soft iron*, $\frac{1}{70}$ of an inch in diameter—treated as the steel wire, being cleaned and stretched on the $5\frac{1}{4}$ feet rod. Before the magnets were applied, it was beautifully free from Magnetic charge. With the magnets it came out as the steel—only it did not hold or retain such a degree of charge in the consecutive poles.

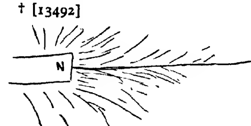
13496‡. As to *conduction* along the wire, there was some power but not very much. Thus, the figure representing the magnet, test needle and wire full size[1], when the end *n* was drawn aside, the test needle M took up a certain position under the power of N; but when the end *n* was brought near M, it deflected it, repelling *o*

[1] Reduced to $\frac{3}{4}$ scale.

* [13492]



† [13492]



‡ [13496]

and attracting *p*, but not very strongly. The lines of force in the air were nearly as influential as the lines of force *in the end* of *n* and issuing at its extremity (13500).

13497*. *Soft iron wire, thick*, being $\frac{1}{30}$ of an inch in diameter. A length of $8\frac{1}{2}$ feet stretched on the long wooden rod—cleaned and examined, was well clear of magnetism. The two magnets applied by juxta position only (13484) at the ends. On examining the wire in this state, it was found that near the poles the wire had like state with the poles to a distance of 10 or 11 inches, that after that the state passed at one end gradually into the neutral condition: but that at the other end the polarity changed two or three times, S, N, S, N, S, etc. at short intervals before the neutral part was reached.

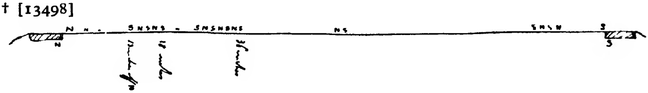
13498†. Took off the S magnet, and beginning at N, drew it as on a former occasion (13485) from N to S in contact with the clean wire and so restored it and fixed it in its first position at S. This drawing of the pole along the wire was done steadily and slowly, so as to give the wire a full charge. Now on examination there were many consecutive poles and apparent irregularities, which the rough figures below will indicate.

13499‡. On taking off the magnets and again examining the wire, the parts close to the poles had changed their sign as before (13493), but the wire was full of irregularities. So magnetic poles promise nothing as regards magnetization of long wires. Try helices.

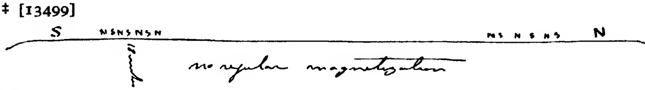
13500§. As to conduction of magnetic power (13496) with a portion of this soft iron wire, it is perhaps better understood by the following expts. If a piece of the soft iron wire of $\frac{1}{30}$ diameter and 18 inches long, perfectly free from magnetic charge, be placed

* [13497]

† [13498]



‡ [13499]



§ [13500]

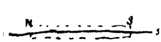
with one end against the pole N, it will become N throughout its whole length, but in hardly a sensible degree at the end *e*. Its N-ness will increase from thence to the pole N. If it be then taken away from the magnet and examd., its end *a* will now be a strong S pole; this kind of polarity will diminish and change until at about *b* will be found a strong N pole; towards *e* that N-ness will quickly diminish and be almost insensible at *e*—but there will be no other consecutive poles. If *then*, the same end *a* be brought against an S pole, the reverse effects will take place; all the wire will be S whilst in contact thus with the pole, but on separating it from contact, *a* will become N, which will change to S at *b*, and that S-ness will diminish further on to nothing.

13501. However often the end *a* is thus changed from pole to pole, the same effects will occur and no consequtive poles beyond those described will occur. All this is simple enough. But if the wire be turned end for end, then all becomes complicated, because of the superposition of a charge at each end. These in a long wire never meet in the middle sensibly. In a short one they may overlap and where a magnet is drawn along from one end to the other. Very slight circumstances of contact, etc. may cause much variety and many consequtive poles.

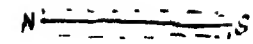
13502. Preliminary expts. on mode of magnetization by a helix, which being a hollow cylinder had external diameter of ·85 of inch and an internal diameter of 0·7, and a length of 7 inches. The wire, copper, 77 feet in length in two layers.

13503. Soft iron wire $\frac{1}{30}$ of inch in diameter was used, and in each case was quite free from previous magnetism. Various lengths were cut off, introduced into the helix, a current of 5 pr. Grove's plates sent through the helix—then stopped—they were taken out of the helix and then examined by the small magnetic needle (13483). The place where the helix has been applied is marked by dotted lines.

13504. A piece of 10 inches long was magnetized simply, the chief N- and S-ness being at the place of the ends of the helix; a lesser degree of force of like nature appeared quite at the end. There were no consecutive poles between N and S. Whilst in the helix and the battery force on, the disposition was the same but the force more powerful. The helix itself has like power which was added to the power of the iron core.

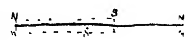


13505. A piece 7 inches long—the poles then quite at the end.



13506*. When a given compass with a needle half an inch in length was in the line of the helix and 7 inches from one end, the helix being at right angles to the needle, then on sending the current through, the action of the helix was to deflect the compass about 40°. Put in 12 inches of the iron wire thus†. Whilst in the helix and the current on, the action was on the compass as before, neither more nor less; as if the iron end *n* had no effect on it, i.e. not more S effect than the helix alone. When the iron was out of the helix, the N and S poles were as marked, but the end *n* was a little North and not S.

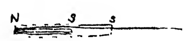
13507. The wire was now reversed and placed in the helix in the same position as before, as shewn by the dotted letters. After a momentary contact with the battery, it was again examd. and found to be as shewn by the upper full letters. So the part within the helix had been overpowered and uniformly arranged without consecutive poles, but the part outside was but little disturbed. The helix governs the inclosed part.

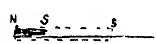


13508‡. A length of 18 inches of the same iron wire was placed uniformly in the helix—the chief places of N and S are marked, but these qualities continued on to the ends. Had Nn been away, s would according to the former experiment have been n (13506).

13509§. A length of 24 inches of the same iron wire was placed in a like position in the helix—the result was nearly the same; the extreme left end was a little n but the extreme right end was indifferent or a very little n also. No consecutive poles.

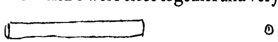
13510. Bent up the long wire thus and put it in the helix. It was found afterwds. as marked, each longitudinal portion having its due polarity—but no consecutive poles. Bent up the part N S so as to make it eight-fold. The same general result—no consecutive poles.

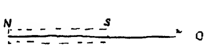




13511. Placed a piece of like steel wire i.e. $\frac{1}{30}$ softened in the helix. Its length was 7 inches. It came out very irregular and with many consecutive poles. Examine further to-morrow.

13512. Worked with *Steel* wire about the $\frac{1}{30}$ as before with iron—the steel soft. A piece 7 inches long became as marked. The intermediate N and S were close together and very strong. Reversed

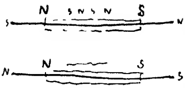
* [13506] 

† [13506] 

‡ [13508] 

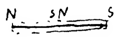
§ [13509] 

the piece of wire and found the new state induced just what the first had been and the two middle consecutive poles.



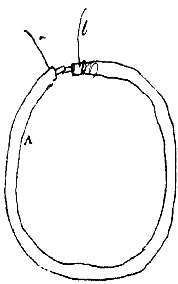
13513. A piece 12 inches long–became charged as indicated. This being also reversed, became alike from the chief place of polarity to the ends, and the intermediate parts had only a very few and feeble alterations of force.

13514*. A piece of 18 inches long was polarized very gradually and well–little or nothing in the intermediate parts.



13515. A new piece of 7 inches long had as before two consecutive poles in the middle. Another piece of 7 inches long also had them, but a little nearer one end than the other. A Long piece of iron wire $\frac{1}{30}$, 4 feet in length, had two helices placed conformably on it at the middle† and when all was connected, they were drawn to the two ends. The chief polarity was at the ends–the intermediate parts had very little external. Neither did the opposite poles appear to any degree at x, x. The whole seemed to be one long magnet–only of soft iron.

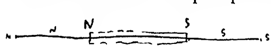
3 NOVR. 1854.



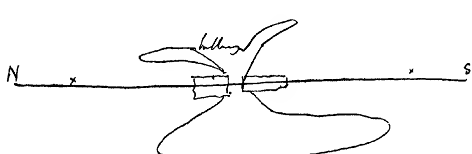
13516. A Gutta percha tube 0·3 of inch external diameter and 33 inches long had about 75 feet of copper wire $\frac{1}{30}$ in diameter put in a close spiral around it from end to end in about 700 spirals, forming the flexible helix A. A *soft iron* rod 33·6 inches long and 0·15 of inch in thickness was put into it and bent round so that the ends could touch. Then a Grove's battery of 5 pr. of plates was connected with the ends *a*, *b*, after which the latter were removed from the battery and connected with the extremities of my Ruhmkorf's Galvanometer ( ). Lastly the ends of the magnetized core were opened and the Galvanometer needle moved a very little, due to the fall in the charged state of the core consequent upon separation of the ends. When left a few minutes, putting the core ends together again did not give any sensible reverse current. The effects were small–probably a thicker wire Galvanometer would have done better.

13517. A like core of soft steel wire was employed and with like results. The effects were perhaps a little stronger. The results of

* [13514]



† [13515]



the fall of Magnetic charge are not so strong as to give a very workable effect with this galvanometer.

13518. In examination of the effect as to induction produced by putting the ends of a magnet into connexion by a mass of soft iron, so as to approximate the magnet to the condition of a ring iron core, when the lines of force are supposed to be chiefly if not entirely *within* the magnet—I took a horse shoe magnet (the smaller horseshoe of five associated bars ( )) and roughly wound 15 feet of covered copper wire about the two poles; then connecting the ends of this wire with the Galvanometer (13516) and all being at rest, the keeper was suddenly put in place and the needle deflected from its position about 20°. When at rest again, the keeper was suddenly removed and the needle was again deflected in the reverse direction, about 17°. These results occurred again and again. Suspecting the needle was (in the haste) not carefully adjusted at zero, the connexions with the Galvanometer were changed, and then putting up the keeper caused a deflection of 20° and taking it away one of about 17°, of course on the same sides as before.

13519. So putting on the keeper to a steadfast magnet so charged with a helix induces a current—taking it off induces a contrary current of equal force.

13520. Now the keeper being away and the needle at rest, the copper wire helix was slipped off the magnet. This of course induced a current and it was more than that produced in the former cases; perhaps the deflection was 25°; but the experiment was only roughly made.

13521. Taking away the keeper or the helix produced a current in the same direction. Putting on the helix or putting up the keeper produced a current of like direction, but contrary to the former.

13522. I have a ring core made of soft iron wire (13516), and it is surrounded by two helices. I sent the battery of 5 pr. current round one of the helices, and of course the iron ring core became a magnet; but all its power was not within, for a needle on the outside was considerably affected. Some parts shewed an outward northness and other parts an outward southness. Unless the iron is very equable and also unless the helix is very uniform in every part, the core becomes irregularly magnetized.

13523*. The 15 feet of copper wire (13518) was made into a more compact helix of 30 turns on a card fitting on to one of the limbs of the horse-shoe magnet—it was then permanently connected with the wires of our large lecture room galvanometer. Being put on to the magnet at *a* it produced a current through the galvanometer—when moved from *a* to *b*, a like current of the same kind was produced—when moved from *b* to *c*, the same result, and even when moved from *c* to *d*, there was a like current. This shews that the helix intersected lines of force all the way down to *d*. The current at *d* was very weak—that from *a* to *b* the strongest.

13524. Putting the helix on the magnet to *a* produced a current. Then connecting N and S by soft iron produced a strong like current. This is because lines of force before passing through the air between the limbs at *e*, *f*, *g* and *h* are now gathered up and pass through the iron keeper. The gathering in of these lines across the helix is just as if the helix had been passed from *a* to *d* (13523).

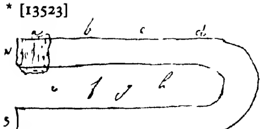
13525. So when the helix was first at *d* and then the soft iron keeper applied to the poles, there was a little current in the same direction but only a trace.

13526†. Arranged two circular bar magnets as figured and two connecting pieces of soft iron. Put a helix on one of the magnets and connected it permanently with the galvanometer; it was too long but was the most convenient one that I had.

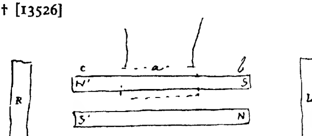
13527. When the helix was at *a* in the middle of one magnet, and the keeper R closed up to N′ S′, a current was produced of a certain amount of force. Then closing up the other end by L, another current in the same direction and of the same amount was produced. When both ends were closed up at once, a like current of apparently double the amount was produced.

13528‡. When the helix was at *c* and then that end was closed by the soft iron R, a like current to the former was produced—strong—nearly as strong as when both ends were before closed at once (13527). Then on effecting the closure at the other end

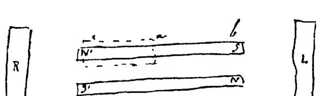
* [13523]



† [13526]



‡ [13528]



by L, a very feeble current in the like direction was produced. On the other hand, if both ends, being open, the L end was closed first, then there was a like current of small amount–but on afterwds. closing the R end, a good current in the same direction occurred. It is evident that when one end is closed first, the remaining condition is like that of the horse shoe magnet, closed as before described (13524, 5) with the helix at different places–and the results are like the former.

13529. Whether the helix be at *c* or *a* or *b*, the closing of both ends at once produces a like amount of effect–and the opening occasions an equally like amount of current in the opposite direction.

13530. When the helix was at *c*, joining up at the L *b* end did very little–but then joining at *c* developed a full and equivalent current. If however the joining was at *c* first, there was the full current, and then after joining at *b* produced a little more.

13531. When the helix was at *b* end, then joining at *b* end gave a good swing and full current, and after joining at *c* end produced a little more.

13532. All these things simple and consistent, and I think lead on very simply to the condition of a *ring core*, helix and magnet: but I do not *as yet* realize in that case the conception of lines of force *crossing* the spirals of the helix. They only as it were appear or rise into existence on *one side* of the copper wire of the helix. That may no doubt be considered as approaching to the neighbourhood of lines of force, but perhaps not as an intersection. Yet if a second spiral be outside the first, it can be affected by the lines of force which have all the while been located within the core.

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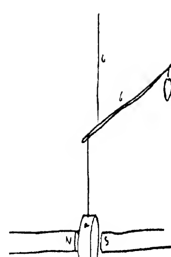
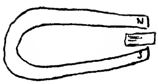


13532[1]. *Movement of copper medium by magnets.* *a* is a copper disc (or rather two discs put together) 1·3 inches diameter and 0·5 in thickness, weighing 1502 grains–suspended by a copper wire to the balanced beam of wood *b*, that being suspended by a bundle of silk fibres *c*, so as to give free vibration. S and N are two bar magnets with opposite poles as in the figure.

13533. In Exp. Res., 2338. Reducing (= taking away) the magnet

[1] 13532 is repeated in the manuscript.

caused the copper block to move as if following it. Would withdrawal of one magnetic pole do the same thing and its approach cause a repulsion? Effect not sensible with the poles as in the figure and one taken away. Neither was it sensible when the two bars with like poles together applied as one pole on one side. But the magnets are not good. Probably when the two were on opposite sides, both would attract as the power of one fell and so neither attract absolutely.



13534. Arranged the disc thus in plan, i.e. put it edgeways between two poles and withdrew them, the disc being sheltered by a screen. There were traces of following on by the disc. In fact the form of experiment now passes into Arago's result, the fundamental principle being still the same.

13535. Removed S above (13532) and made N our cylinder electro-magnet, exciting it by 10 pr. of Grove's plates. The effect was clear: on making the magnet the disc was repelled, on reducing the magnet it, the disc, was attracted. The actions were complicated, each case containing the effect due to diamagnetism and that due to the momentary condition of currents.

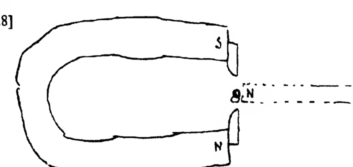
13536. In place of hanging the disc on the torsion balance, I hung it up as a pendulum by a suspension of about three feet; this would separate the effect of momentary attractions and repulsions from those that were permanent. Now on exciting the magnet the disc was repelled out perhaps the $\frac{1}{12}$ of an inch, but instantly fell back again and was hardly sensibly diamagnetic, so little did it remain displaced from its true or first place. On reducing the magnet, the disc advanced strongly towds. the magnet for an instant and then returned back as before to its first place. All of which confirm former results (8418), shew how little the true diamagnetic force is (Matteucci) and how great the temporary effect.

13537. We are pretty sure that the effect of making a magnet and approaching a magnet is the same. Exp. Res., 1st Series.

13538*. *Places of weak force—neutral places* (In development of results in the Exp. Res., 2298, 2487, 2491).

N, S, is the Logeman great magnet, with its two moveable pole pieces. ◐ is a cube or a sphere of bismuth $\frac{1}{2}$ an inch in diameter, suspended on a torsion balance so as to have free motion along

* [13538]



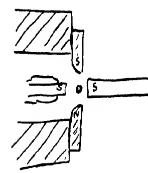
the line between the two poles—its place of rest under the circumstances was midway between the two poles. Made the two bars magnets (with like poles together) as one magnet and put it in the dotted position—when the N pole was thus approached, the bismuth moved towards the N pole of the Logeman as figured. When the S pole of the bar magnet was approached, the bismuth moved towards the S pole of the Logeman—going in both cases towards a weaker place of force.

13539. When the bar magnets were replaced by our large cylinder electro magnet (13535), then the effect on making and reducing the magnet was the same in kind but very powerful—thus when it was excited the bismuth approached almost up to the *like* pole of Logeman's magnet, and when it was reduced went back instantly into its medium position.

13540. Employed the helix of the cylinder magnet only without the iron core—the same results occurred but much more feebly—directions were the same.

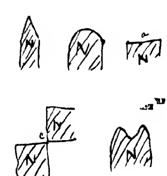
13541. Employed a smaller cylinder electro magnet (core 12 inches long and 1 inch diameter); it shewed the effects well.

13542. Arranged matters so as to be able to place the S pole of the compound bar magnet on one side of the bismuth—it sent the bismuth towds. the S pole of the Logeman. Then arranging the S pole of the smaller cylinder Electro magnet on the other side of the bismuth, it by itself sent the bismuth also towards the same pole of Logeman. When the bar magnet pole was first in place and had sent the bismuth towards the Logeman S pole, then the addition of the helix S pole on the other side sent it still further towds. the same Logeman pole. Which I think takes away all notion of polarity save that of conduction polarity (   ).

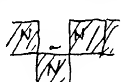


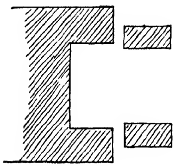
13543. In all these cases the bismuth moves from stronger to weaker places of force. See on (13544).

13544. A point presents a place of strongest force—a spherical end a surface nearly equal in force at different parts—a plane termination is weaker in the middle at *a* than at the edges or circular angle, where it is strongest—two poles put together would be weakest at *c*, for they are like one pole having a depression in the middle. Three poles arranged as figured* would be still weaker at *a*—and so we may go on until we arrive at chambers of no action. The following are illustrations.

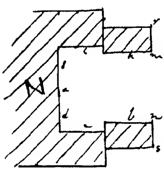


* [13544]





13545. An iron cylinder core 9 inches long and 1·6 diameter has a cylindrical chamber at one extremity 0·9 in diameter and 0·4 in depth—a ring of iron 1·2 external diameter, 0·77 internal diameter and half an inch long, being put on to the end of the core, makes a chamber of an inch in depth—and this depth can be increased to any degree by like cylinders of soft iron of different lengths. A helix of thick wire belonged to this core, being the helix of the large cylinder magnet ( ); in the following experiments, it was excited by 10 pair of Grove's plates.



13546. A piece of iron about this size* held at the end of a piece of copper wire was felt to be very strongly attracted by this pole at *r* and *s*—also strongly but less at *m* and *n*—a little at *k* and *l*—but not sensibly at *a*, *b*, *c*, *d*, *e*.

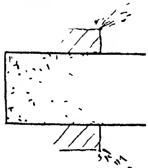
13547. A piece of cartridge paper of this shape† was sprinkled over with iron filings and then put in a horizontal position into the chamber, after which the magnet was excited, discharged and then the paper taken out. Throughout the bottom of the chamber and till near its mouth, the filings were quite undisturbed, but near the mouth they had been carried away by the forces and left at *r* and *s*, the two strongest places of action. The deficiency of power within the chamber was beautifully shewn here.

13548. With the chamber only half an inch deep, there was no sensible attraction at the bottom on the short piece of iron ( ), but with a nail or a longer piece touching only at the bottom, there was much attraction—for it can polarize and convey the force from the interior to the exterior—the dualities in it come into play, though they cannot with a shorter piece.

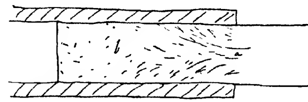
13549. Employed a cylinder iron core 7 inches long, 1·1 external diameter and 0·75 internal diameter, with the same helix and power. The short pieces of iron (13546) seemed to lose all attractive force when a very little way up the inside, whereas a long piece touching a good way up the inside and projecting outside was well attracted within. When a long slip of paper with iron filings was introduced, only those near the mouth were affected, yet the paper had been tapped to make every thing favourable‡.

* [13546] 

† [13547]

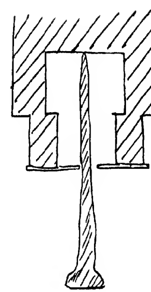


‡ [13549]



13550. A small needle on an axis of cocoon silk, fixed to a piece of card and held by a long splinter of wood, was introduced into the cylinder core whilst it was a magnet. At the mouth of the aperture, at *a*, it was powerfully affected, but became less and less affected as it passed in, and at *b* or $1\frac{1}{2}$ inches was not sensibly affected–its movements were quite slow and just due to torsion and gravity, whilst at the mouth the vibrations were too quick to be seen. The same effects happened as the needle was passed into the former chamber, but there signs of force offered by it, at the bottom of the chamber, though great diminution had occurred.

13551*. The retention of iron filings is another proof of the degree of magnetic power in presence at any spot. The chamber 1 inch in depth (13545) was filled with filings, closed by a card–the core put into the helix–placed in the upright position–magnetized and whilst a magnet the card taken away; immediately the filings fell down; many were caught and hung in bunches from the lower edges of the chamber but not one filing remained attracted against the bottom or inside of the chamber.



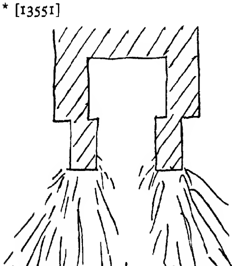
13552. But though the smallest piece of iron cannot be held there, yet if it be exchanged for a longer piece that can come out into the free space, then it is attracted at the bottom and all things are changed.

13553. When the cylinder core was filled with iron filings, closed by a card, placed vertically and magnetized and then the card removed–the first filings that ran down were caught by the edge as a fringe, but all the rest ran out in a stream.

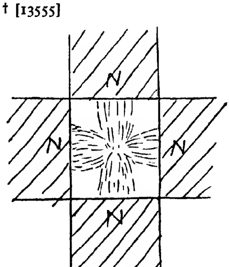
13554. It is easy to see how these results pass into those with the Logeman and two like poles (13542).

13555. I have four very hard bar magnets; each is 6 inches long, 1 inch broad and nearly 0·4 of an inch thick. With these it was easy to build up chambers having the properties of those just described. They were arranged thus†, on a horizontal plane, having

* [13551]

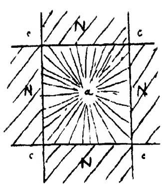


† [13555]



a piece of card board placed in the middle square space midway in the thickness of the magnets. Iron filings were then placed carefully on the card board or else put on it first, and then the magnets regularly approached. As they came up, the filings formed radii from the center of the card board to the edges against the magnets, and by careful tapping they ultimately arranged themselves as in the figure. The four S poles produced the same effect.

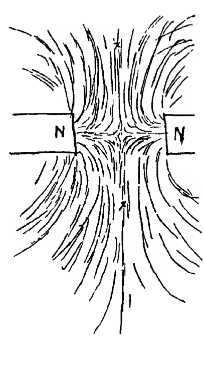
13556. Here it is easy to see that the lines of force proceed from all sides inwards in this central plane, to turn up and then over and outwards above the plane, and in the reverse direction below it. The lines of force are stronger and more direct from the middle of the sides than at the angles—the cause of which is again easy to see, for at the angles there is distributing space back in the same plan[e] as well as above to a far larger extent than at the middles, and part of the northness gets disposed of there, through the corners of the magnets as it were.

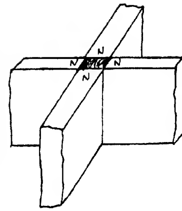


13557*. When a little needle (13550) was applied to the magnet chamber made up in the air and with the card board removed, it shewed beautifully the lines of force as radii to the very center in the *medium plane*; then there was a double axis, i.e. northness proceeded from *a* upwards and downwards, rapidly decreasing in force. But when the needle was raised above the medium plane, it was seen that the lines of force according to their places soon took a direction the reverse of the former, proceeding back to the S poles—and all the lines of force from the outside angles *c*, *c*, *c*, *c*, were also in that direction, i.e. north outwards and toward the souths. When the axis of exit *x x* shall be stopped by two more poles, as in the case of the six magnets, making [ ], then these lines will disappear or nearly so from the cubical chamber formed.

13558. I placed the poles together edgeways so as to make a

* [13557]





narrower and a deeper chamber, and then introduced the needle. The disposition of the power near and about the upper and lower aperture of the chambers was as before, but in the middle of the chamber there was scarcely a trace of power, though the needle was very sensitive. There was a place of ***no action*** almost. Very beautiful. It is easy to see how this coincides on the one hand with the cavity in a pole (   ) and on the other with the chamber of six poles (   ).

13559*. *Solid copper globe revolved.* The globe is   inches in diameter–solid–mounted on a copper axle and revolving in a lathe standing nearly magnetic north and south. The rotation of the ball therefore was in the plane of Magnetic East and west. This would cause electric currents to flow through the top and bottom of the ball in a manner analogous to those occurring with a brass ball in my old experiments. The effect however on the needles prepared was not so good as I expected, and I suspect that much of the middle part of the globe is discharging currents produced in the upper and lower parts.

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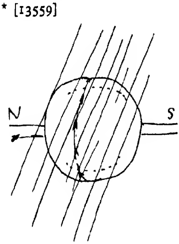
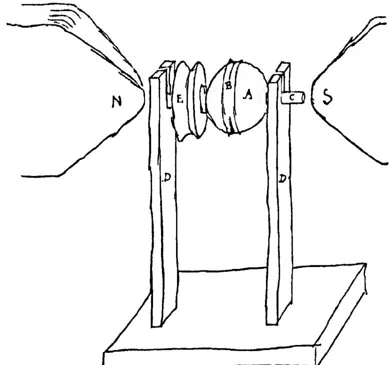
13560†. A, a globe of any particular metal with a thin copper equatorial ring B driven hard on to it, grooved to hold a wire. C is a copper axle held in the supports D, D. E is a pulley on this axle by which motion can be communicated to the globe from a large wheel and gut or cord. This pully has a diameter of 0·8, the large wheel of $17\frac{1}{2}$ or 22 times–N, S, are the sub poles of the Logeman magnet (the great magnet), which acts well with such spheres as that described. There are four of these spheres,

* [13559]



† [13560]



all of the same size, *Copper*, *Iron*, *Bismuth*, *Hard Steel*, each with its own equatorial ring of copper and its axis of copper and its wooden pulley. A clamp holds the stand D, D, to the table and then the Magnet is adjusted to the place of the Globe.

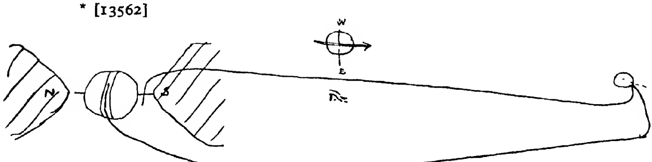
13561. The Galvanometer employed is my Rhumkorf, placed about 13 or 14 feet from the magnet. The wires of the Galvanometer are in the experiments held at C and at B in order that the currents produced by the moving radii may be carried off and read at the instrument. One of the wires is a little thicker than the other; the thick wire is *always applied at the Equator B* and the *thinner wire at the axis C*—the contact is a mere friction contact of copper against copper in all cases. The Galvanometer stands with the needle end north and south; the *South end* is that with the graduation and its motion shall always be spoken of as to the *East* or to the *west*. The axis of the revolving globe is also nearly north and south; looking at it from the *north side always*, its direction of revolution shall be spoken of as *with* or *against* the clock.

13562*. *Bismuth globe* in—revolved with watch—the S end of the Galvanometer needle passed *westward*. Revolved against the watch—the S end of needle went East. So the effect not a thermo current from friction. Changed for this time only the contact of the wires at the equator and axis of the revolving bismuth, and now when bismuth revolved with the clock the needle end went East—and when bismuth revolved against the clock the needle went west. Four revolutions of the large wheel (13560) were made in each of these experiments and the deflection of the Galvr. needle was about 5° to the West and 10° to the East. The difference is due to thermo current due to the heat of friction at the equator of the bismuth globe.

13563. Resumed the standard movements (13561). Ten revolutions of the large wheel gave higher results, but complicated with thermo currents—10 revolution[s] *watch* fashion gave only 5° to West—10 revolutions *against Watch* gave 60° to East; the mean of these, or 33° about, must be taken as the effect due to the rotation induced current in the bismuth globe.

13564. If only a few moments after, the galvanometer wires were applied to the bismuth globe quiescent, no sensible signs

* [13562]



of a thermocurrent were obtained, for then the heat evolved by friction at the copper equator had been conducted away into the mass of the globe.

13565. To obtain a *standard of rotation* for this and other results, the galvanometer wires were joined and then moved between the poles so as to represent the upper radius of the revolving ball—when moving *with the watch*, the needle end passed *West*—when the motion was against that of the watch, the needle end went East. So copper and bismuth the same.

13566. A *soft iron globe* (13560) was put into place and experimented with; the revolutions are those of the great wheel (13560).

4 revolutions Watch fashion sent the end of the needle 20° West
4 Do. Reverse Do. 25° East

The results were so clear and good that no doubt can arise about them—the thermo effects are of course less than with the bismuth.

13567. So *Bismuth*, *copper* and *iron* are alike in direction of current caused.

13568. Removed the Sub poles, leaving the chief magnet and the iron globe in the same places; the distances were as above*[1].

4 Revolns. Watch fashion produced 5° *West* deflection
4 „ Reverse watch „ 8° *East* deflection

Made x distance 13 inches by removing the magnet; then

4 Revolns. Watch fashion produced a trace of *West* deflection
4 „ Reverse watch „ Do. *East* deflection

The iron has evidently retained little or no magnetism, or it would have given its own effect here.

13569. Now worked with the *hard steel globe* (13560) and first put it into the place of the iron just left, i.e. 13 inches distant from the axial line or face line of the poles; the globe being examined by a magnetic needle was found to be unmagnetized—

4 revolns. Watch fashion gave needle deflection a mere trace West
4 „ Reverse watch Do. Do. East

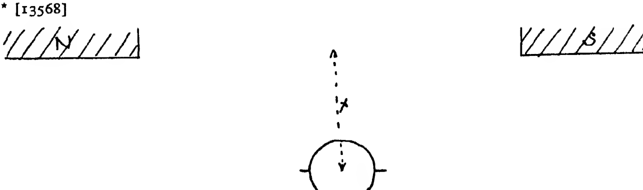
13570. The magnet brought up into the medium iron position (13568).

4 revols. Watch fashion caused deflection 6° West
4 Do. Reverse watch Do. 5° East

13571. The globe being taken away from the magnets and

[1] The diagram is reduced to $\frac{3}{5}$ scale.

* [13568]



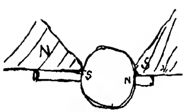
examined by a needle, was found as yet almost entirely free from magnetism.

13572. So Bismuth, copper, iron and hard steel all give the same direction of induced current.

13573*. The sub poles of the magnet were put in place with their extremities 3·3 inches apart and the hard steel globe midway.

4 revols. watch fashion . . . deflection 13° West

Being taken out an[d] examined, the magnet [globe] was found to have received a little magnetic charge—not quite regular—so little that it caused no current by revolution when the magnet was taken away.



13574. Made the Steel ball a magnet by bringing the sub poles N and S up against it, turning it round on its axis and jumping the poles mean while against it, so as to charge it in the best manner possible. When examined by a needle afterwards, it appeared to be a pretty good short magnet. Being mounted on the frame but revolved by itself and away from the power of the Logeman, it gave its own result, namely:

4 revols. Watch fashion . . . cause deflection 6° West
4 revols. Reverse watch . . . Do. 5° East

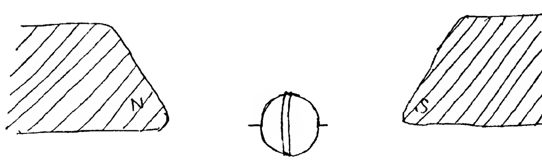
13575. The copper equator of this steel magnet is not tight, nor the contact so good as it ought to be; nevertheless the results are quite accordant in direction. For as the globe magnet stands in the position in which it was made a magnet, so the results should be in the same direction as with the magnet.

13576. Brought up the great magnet into the position (medium) of the iron globe (13568), so that the influence of the great magnet and of the globe magnet should coincide.

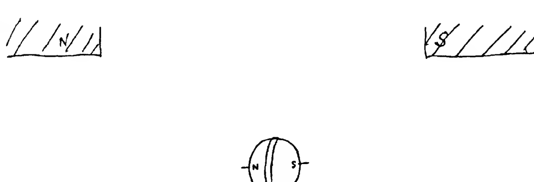
4 Revol. Watch fashion caused deflection 9° West.

13577†. Reversed the globe magnet, the position being in all other respects the same.

* [13573]



† [13577]



4 revol. Watch fashion caused deflection 3° West
With the magnet a little further off:
4 revolutions Watch fashion caused deflection 1° West
The magnet a little further removed:
4 revoln. Watch fashion caused no sensible deflection
With *the Magnet away*:

4 revolutions	Watch fashion	caused deflection	4° East
4 Do.	Reverse watch	Do.	2° West
4 Do.	Do.	Do.	2° West
4 Do.	Watch fashion	Do.	4° East

13578. So the globe had remained a magnet all the while—at one distance its whole system was compressed within itself and there neither its power nor that of the great magnet was manifest—the place as a whole was a place of indifference. Inside of this place the great magnet overpowered the globe, giving the difference of force as the result. Outside of this place the globe overpowered the great magnet, and so gave again the difference with a change in the direction.

13579. Restored the great magnet to the position (13577), the globe being still reversed:

$\left.\begin{array}{l}\text{4 revols. Watch fashion caused deflection 6° West} \\ \text{4 revols. Reverse watch Do. 6° East}\end{array}\right\}$

These are results of the great magnet overpowering the globe, but when the globe was taken out and examined, it was still found magnetized as originally charged.

13580. Then experimented using my thick wire Galvanometer and found excellent results—perfectly accordant with all that had gone before. With the *Soft iron globe*, half a revolution of the great wheel, Watch fashion, caused deflection 90° West of the graduation or N end of needle.

13581. Bismuth globe in:

½ revol.	Watch fashion	caused deflection	8° West of same end
2 Do.	Do.	Do.	90° and more West
1 revoln.	Reverse watch	Do.	90° East
1 revoln.	Watch fashion	Do.	1° West only,

for 22 revolutions of the equator (13560) under the wire of contact now produced so much heat as to give a thermo current counteracting that of the rotation when revolution was with the watch, and adding to it when revolution was reverse of the watch.

13582. Found that our large thick wire galvanometer would shew all these results.

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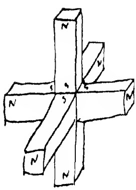


13583. Have formed a magnetic chamber with Six like poles, thus. Six cores of soft iron, 3 inches long by 1 inch square, with rounded edges—each has had 128 inches in length of covered copper wire 0·05 of inch in diameter wound round it in 32 spirals or coils. These are fixed on a wooden frame so as to be held pretty firmly in their places, and then the wires soldered together in one continuous series, so that when the electric current of 10 pr. of Grove's battery was sent through them, all the inner poles were S poles, or else all N poles.

13584. By separating the poles a little, access could be had to the interior, and so the effects on this or that test observed; but the effect of such openings was always evident.

13585. A Little magnet supported by a single cocoon thread was weighted with a little copper wire, without which it could hardly be kept from the iron at the edges of entrance when the iron was in its unmagnetized state. The frame and magnets were tilted, so that it might hang in the middle of the chamber. When there, it was not affected when the magnets were excited; but if nearer to the apertures, then it shewed a little pointing action. But it was as nothing to the amount of force on the outside of the chamber, even when the needle was several inches away from the nearest part of the system.

13586. When the currents were taken from the electro magnets, they sank exceedingly well.

13587. A crystal of bismuth was suspended in the chamber; it gave no magnetic indication of any kind—neither old nor new.

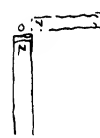
13588. A little piece of iron hung at the end of a jointed piece of copper wire—shewed no sensible traces of attraction within the chamber, except near the edge openings. At the outside it was powerfully held.

13589. Filings on a card were introduced, then the magnets made and unmade and the filings examd. Those in the middle part of the chamber were undisturbed though the card had been tapped—

those near the edge apertures and also middle of the side were a little arranged.

13590. A small helix ring of several convolutions of wire was revolved in the chamber—and with it were connected at different times two commutators, changing at different periods. My Rhumkorf and my thick wire Galvanometer were both used (the latter is the best), but by no arrangement could any but very feeble results be obtained, due to the disturbance of power at the opening of introduction. When these helices were brought near one of the Six outside poles and revolved, then abundant currents of electricity were obtained.

13591. *Bismuth* and *like magnetic poles*—a cube of bismuth suspended by a fine wire 13 feet long—N the cylinder electro-magnet; when excited it drove the bismuth O away; then another electro magnetic N brought up at the side caused the bismuth to approach again, but the suspension is not long enough and the effects are small—the torsion balance suspension is better (13538).



FOLIO VOLUME VII
OF MANUSCRIPT

Magnecrystallic force.

13592. I have a *prism of bismuth* cracked out of a lump—it is eight sided and 0·45 of an inch long—its weight is 77 grains—its prismatic form is rather irregular, and there is a small oblique piece off at both ends. Its magnecrystallic axis is perpendicular to its length, and to two of its sides; so that when it is suspended vertically in a field of equal magnetic force, it sets by the difference of magnecrystallic force in two directions. When it [is] suspended with the axis vertical, it sets as a long piece of diamagnetic matter, and when it is suspended by one side with the axis horizontal, it sets with the combined diamagnetic and magnecrystallic force united.

13593. I have also a sphere of Iceland spar given to me by Professor Thomson. It is 0·67 of an inch in diameter and weighs 103·7 grains: is polished and the optic axis is indicated by a very small flat plane.

13594. The Logeman magnet is set up, and the pole pieces of iron are used, having this shape*; the face *a* is 1·7 inches square. There is a little screw hole in the middle, but it will not interfere seriously with my experiments.

13595. The piece of bismuth was used and *a silk cocoon fibre suspender*; there were ten fibres and the length only 5 inches, so the torsion of such a bundle is considerable. At the bottom of the silk was a little piece of card—sealing wax—copper wire and soft cement. It pointed between the poles when they were placed an inch apart but was employed for the first trial experiments. The soft cement held the bismuth or the sphere perfectly well for the time by an adhesion not the $\frac{1}{20}$ of an inch in diameter.

13596. The bismuth was suspended vertically in the middle of the magnetic field, pole faces one inch apart. It would set twice in one revolution because the force is axial; I shall therefore always count the half revolutions, i.e. each 180°, as a unit of motion either with or against the watch. Being in place, the bismuth was set round in the direction of the watch by half turns until the torsion of the suspender was all but enough to overcome the

* [13594]

magnetic set; and when at that point, the bismuth was then moved against the watch motion until it had acquired the same balanced condition on the opposite side—the half of the number of units shewed the power of torsion (in a rough manner) exerted by the magnecrystallic force under the circumstances. Now with the end up, it required 17 half turns or units between the balanced condition on the one side of no torsion and the balanced condition on the other. This was repeated two or three times very regularly—$8\frac{1}{2}$ is therefore the torsion.

13597. Now the bismuth piece was suspended with the length and the magnecrystallic axis also horizontally—and the process was repeated. It required 39 units or half turns now to raise the torsion to the balancing point, so that $19\frac{1}{2}$ is the required torsion.

13598. When the bismuth was off and away, the copper wire, etc. at the bottom of the suspender could point and bear several turns, and though its weight is small and therefore these turns bear no proportion in torsion force to that when the bismuth was on, yet the copper, etc. was removed and a piece of clean soft cement attached to the card. This wax, etc. did set a little, but its power was not equal to a half turn of the suspender. Hence there was very little interfering directive force here.

13599. Bismuth prism (13592) on with one end upwards. The number of units or half turns from extreme to extreme was almost if not quite 20. Again in the opposite direction, and again almost 20. Tried a third time, and still almost 20. So that 10 may be taken as the amount of torsion needful to balance the magnetic force in this position.

13600. Placed the other end of the bismuth upwards. First observation gave 20 units—also the second in the reverse direction 20 units—and the third also 20 units. Beautifully accordant, so that 10 is the torsion force. It appears almost certain that the copper, etc. of the first suspender had been placed across the magnetic resultant, so as to lower it a little in the first trials.

13601. Now tried the Magnecrystallic power of the sphere of Iceland spar (13593), using the same suspender. The optic axis was placed vertically. There was no set except by torsion—no apparent magnetic action. The *optic axis* was placed horizontal, and now there was *set*, but the power was small. The sphere

would bear *one half turn* but not two. Turned the pole pieces (13594) and used the pointed ends near together—but still, from extreme left to extreme right did not amount to two half turns.

13602. Prepared a suspender of less torsion force. It consisted of four fibres of cocoon silk and these 9 inches long. With this suspender and the pointed poles, the sphere bore 4 half turns between the two extremes, and almost five. *So this will do.*

13603. Took a little rhomboid of Calc. spar—suspended it with the optic axis horizontal and employed the flat faces of the pole pieces. There was very little set, for the diamagnetic force of the lengthed. direction and the optic force of the shorter direction were almost equal: the optic force was a little the strongest.

13604. As I want copper wire for a bismuth suspender, I examined a fine wire—cleaning it well; a bundle 0·3 of an inch long, containing 30 turns of the wire—suspended horizontally by the more delicate suspender (13602) set and gave 10 half turns between the extreme sets in opposite directions, having only its own weight on the suspender.

13605. The bismuth prism was suspended by *one* turn of this wire round it, the bismuth being vertical and the wire in the plane of the chief magnetic axis. The flat pole faces one inch apart. The number of half turns between the extremes was 20, being the same as those when no copper was employed. Then the bismuth was removed without altering the form of the copper suspender and it was put alone into the field; it could not sustain one half revolution of set now, though there was scarcely any weight on the suspender. So that this copper will do for the bismuth in phosphorus, etc.

13606. Melted phosphorus under water—put bismuth into it—no action—no tarnish—will do very well. Bright copper wire into the water and bismuth was not affected—will also do.

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13607. As I shall want a water bath in place, I have used the copper cell (   ). It makes the guage between the poles, being just an inch wide in that direction, and is supported by blocks on each side standing on the magnet terminals. It remains always in place

and having a large top above, that serves as a support for other things, as a small thermometer or a stop.

13608. This stop is simply a two pronged fork of copper wire on a foot of lead; it stands on the top of the cell. The bismuth is suspended by the wire before described (13604, 5), which took one turn about it and was then continued upwards for 6 inches—at that point it was attached immoveably to the little piece of card at the lower end of the suspender (13595), being there fixed by sealing wax, and about half an inch of the wire projected horizontally as an arm to serve as an indication of the position of the bismuth. This arm was parallel nearly to the magnecrystallic axis and could vibrate between the prongs of the stop. The latter could be set so as to be just on the outside of the positions of equilibrium due to the force of the magnecrystallic axis and the extreme torsion force, in consequence of which the adjustment of the torsion could be more readily obtained than without such means.

13609. The *bismuth in air*, the piece being vertical and consequently the magnecrystallic axis horizontal. The temperature about 60° F. The Logeman magnet—the square face pieces (13594) and these one inch very nearly apart and constantly the same. My object was to put so much torsion on to the bismuth as to bring it up to the very verge of instable equilibrium on the one side, and then, starting from that, to reverse the torsion motion so as to undo the first and put as much as possible on to the other side, until the corresponding position of instable equilibrium was obtained. Then to start from that point as a commencement and put on torsion in the reverse direction for a second observation and so on. The torsion was given and counted by a graduated circle and index fixed on a stand over the magnet and cell, to which the upper end of the suspender (13595) was permanently fixed. Its action was satisfactory. I shall now count or estimate the torsion by *entire* revolutions of the index and degrees.

13610. So torsion (against the watch) was put on until the bismuth was just on the point of swinging round, i.e. the extreme position between magnecrystallic force and the torsion force was obtained. Then the torsion index was reversed, i.e. moved with

the watch, until the extreme position on the opposite side was obtained.

The torsion required was			$11^r\ 90^\circ$
Again,	,,	,,	$11^r\ 80^\circ$
Again,	,,	,,	$11^r\ 120^\circ$

Having learned in some degree the use of the stop and how to employ it so as to avoid rebound of the bismuth index, three other results of bismuth in air gave

First	,,	,,	$11^r\ 110^\circ$
Again,	,,	,,	$11^r\ 130^\circ$
Again,	,,	,,	$11^r\ 120^\circ$

The average is $11^r\ 120^\circ$, or 4080° from extreme to extreme.

13611. The apparatus is in my sitting room. The carriages cause much vibration, and so it is difficult in air to bring the bismuth up to the extreme point of equilibrium or that of instable equilibrium, because of its toppling over through the vibration and tapping of the bismuth indicator against the stop; but in water things will be better.

13612. Found also that the silk suspender took a set, i.e. that if the torsion were put on on one side to the extreme degree, so as to seem sufficient to reach the extreme point, that if all were left, it did not remain sufficient and that more required to be put on, which by waiting would perhaps amount to 100°. But the first effect was nearly alike in all cases, and by waiting this set came on very regularly as to time on both sides, except that when carriages rattled by they hastened it – 10 minutes or a quarter of an hour generally brought it nearly to a close. The set was of course first on one side and then on the other. After this, it was always waited for, and nearly equably as to time and effect in all case[s]. When it became not more than 5° or 10° in 5 minutes – the observation was made. The preceeding result with air is probably underrated because these things were not then fully learned.

13613. The bismuth (13592) in *pure water* contained in the copper cell. Here vibrations from carriages were much deadened and the bismuth itself held steadier in its motions. It wanted more torsion to carry it to the first extreme and the evident deflection of the Magnecrystallic axis from the magnetic axis was greater than in

air—i.e. it could be attained and sustained better in water than in air—without toppling over. The effect of the waiting was also here more steadily remarked because of the greater facility of observation with things in the quiet state.

13614. In order to get an extreme effect of set, the torsion was brought up on one side, the set waited for a good while and the torsion then brought up, and then the torsion was reversed as quickly as possible and an observation made at once—it gave 12^r 90°; then the torsion unset and counterset was waited for full 20 minutes—at which time the observation gave 12^r 250°, or 140° of difference—half of which, or 70°, is the effect of set from the zero or medium state of the suspender up to the extreme state. This set is allways allowed for, i.e. is allowed and included in the following experiments. The numbers given shew in fact what the index shews, and that consists partly of torsion force and partly of set compensated for.

13615. So the *Bismuth in pure water* gave . . 12^r 250°
Again, ... 12^r 220°

13616. The *Bismuth in Absolute Alcohol.*
At first, ... 12^r 220°
Again . . . . 12^r 180°
Again . . . . 12^r 220°

13617. The *Bismuth in Carbonic acid gas.*

I have had a C.A. Gas generator connected with a small glass tube proceeding down the side of the copper cell to the bottom and I found it easy to fill the cell with C.A. Gas and to keep it so, or to blow it out and have air there—or to fill it again with C.A.

In C.A. Gas . . . 12^r 80°
threw in Air, displacing the C.A. . . still 12^r 80°
C.A. Gas again . . 12^r 80
C.A. Gas again . . 12^r 100° by
waiting [illegible].

13618. The *Bismuth in Phosphorus* (13647). A glass tube was put into the copper cell (13607) and the latter filled with water and made to serve as a water bath—a little thermometer was placed in the water. Pure clean transparent uncoloured phosphorus was put into the tube so as to fill about ⅔ of it, and all the part between

the magnetic poles and over the phosphorus was distilled water. Then the bismuth was lowered into the water—a spirit lamp applied to the bottom of the copper cell until the contents had reached 120°–130° F., at which time the phosphorus was freely melted; after this, heat was applied from time to time and the temperature retained between 120°–130° F. On lowering the bismuth into the phosphorus, there appeared to be a little action, for a small bubble appeared now and then which smoked on reaching the air and breaking, and a few black films rose up as if phosphuret of copper or perhaps bismuth had formed—but the effect was very little.

13619. It required care to keep the bismuth in the middle of the phosphorus; for as it did not wet with the phosphorus, when it came by any accident or motion against the side of the copper cell, the phosphorus divided there by the force of capillary attraction and the bismuth and copper vessel stuck very strongly together. In the middle of the glass tube and of the phosphorus, the bismuth had very free motion. The results were as follows.

First time	11^r 200°
Again, much waiting . .	11^r 270°

The torsion seemed to be very clearly less than in air, water, alcohol, etc. The diameter of the phosphorus medium however is small and it will be well to have a larger tube.

13620. On raising the bismuth out of the phosphorus into the water above, it was found to have numerous bubbles of gas adhering to its surface—also the copper wire w[h]ere immersed in the water was apparently phosphuretted, having changed colour—the bismuth itself was not sensibly tarnished, or very little—these bubbles were easily displaced by a touch with paper—whether they have affected the result in any way, as to altering the form of the envelloping phosphorus, must be settled by future experiments. I think that a voltaic action has gone on to a small extent between the bismuth, copper and water—aided also by the phosphorus perhaps—but it probably soon comes to a standstill or nearly so when the copper is phosphuretted on the surface.

13621. The phosphorus in its tube was removed; it was very clean except at the top, where some films of metallic black phosphuret floated. The bismuth also came away very free from

adhering phosphorus and easily cleaned from the small particles that adhered in one or two places to it.

13622. The *bismuth* (13592) and sat. *sol. proto sul. Iron.* The solution was placed in a glass tube in the copper cell cleared of water and all at common temperature. The observations began and it was soon evident that a high torsion was required. It went up at once from one extreme position to the other, to 12ʳ—this in a few minutes rose to 13ʳ, and after standing a while 180° were added.

So the first observation gave .	13ʳ 180°	average 13ʳ 197°
Again	13ʳ 220°	
Again	13ʳ 190°	

13623. Washed all well and especially the bismuth and its wire carefully; then put pure water into the glass tube, so:

The *bismuth in water* again—with the usual care, etc.

First result gave	12ʳ 330°	12ʳ 340°
Again, reverse direction . .	13ʳ 0°	
Again, direct direction . .	12ʳ 330°	

Is less clearly than the phosphorus.

13624. The *bismuth in Air again*—in the copper cell.

First result gave	12ʳ 190°	12ʳ 217°
Again, reverse direction . .	12ʳ 220°	
Again, direct direction . .	12ʳ 240°	

13625. When the bismuth is in air, there is more weight on the suspender than when it is in water—and when in Phosphorus the weight on the suspender still further diminishes. Must ascertain the effect of this difference. As the bismuth weighs grains, so when in water, it would require grains added, and when in phosphorus grains added to make the tension the same as in air.

13626. Reference to former expts. with bismuth in Water, Air and soln. Sulphate of Iron. Exp. Res., 2499, 50, 51. Torsion required alike in all.

8 MAR. 1855.

13627. *Buoyancy.* The weight of the bismuth piece (13592) is 77 grains and its Spec. Gravity is 9·8. The weight of the Calc. spar sphere is 103·7 gr. and its Specific Gravity 2·72. The Specific

Gravity of water is 1—of the Absolute Alcohol 0·816—of the Solution Sul. iron (13622) 1·187 and of the phosphorus 1·9. So the buoyancy of the bismuth and Calc. Spar in the different fluids is as follows, and the compensating weight required in each case the same (13692):

	Bismuth piece	Icelandic Spar sphere
Air	0	0
Alcohol	6·4 gr.	31
Water	7·86	38·1
Sol. Sul. iron	9·32	45
Phosphorus	15	72·4

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13628. Have had a new copper cell for bath, larger and better than the former; but it is wider for the tubes and so the power in the field will be weaker. When in place, the flat faces of the pole pieces are 1·3 inches apart.

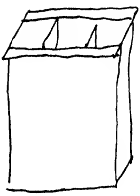
13629. The bismuth piece (13592) with its proper suspender gave from extreme to extreme $6^r\ 50^\circ$ / Again, $6^r\ 40^\circ$ } average 2211°.

Then put on the largest weight but one (13627) 45 grains, and now the number of degrees were diminished, as was expected, for the results, still in Air, came out $5^r\ 50^\circ$ / Again, $5^r\ 0^\circ$ } average 1825°.

The experiments were not very precisely made but enough to shew the effects of tension on the suspender. When the crystal was at one extreme, on taking off the weight, then more torsion was required to be put on to bring the crystal up to the extreme again.

13630. The poising weight or wire being of some size is affected by the currents of air about, so that it is needful to surround it by a paper shade. This I have done and it answers very well. As the weight[s] are only wanted when the object is immersed in a fluid, that will help to steady matters.

13631. Intend also to have two weighted wires as side stays, from the arm over the edge of the table, so as to break the vibration from carriages.



13632. Have improved matters–have a new stop which acts better, not so much vibration–have also strings and weights attached to the support of the torsion index, etc.; they are applied as side stays, one on each side, and do much to break the carriage vibration. Have also a new copper cell–it is inches high and $1\frac{1}{4}$ inch across from pole to pole and inches in the equatorial direction; it has within 2 copper slips going nearly across, but attached only at one side or edge, so that they can be bent to hold the glass tubes which the cell is to contain.

13633. The poles are now further apart than before and therefore the magnetic force less, but the torsion required being also less, the set at the extremes is less also.

Experimented with the *bismuth*.

13634. I have a paper shade also for the upper part of the wire where the counterpoise or compensator will be, and I find that shutting the door (close at hand) does much to prevent aerial disturbance.

13635. The *bismuth* (13592) *in Air*. The copper bath and its glass cell being all in place.

First time 5ʳ 280° from extreme to extreme
Again, 5ʳ 320° " "
Again, 5ʳ 300° " "
} average 2100°.

13636. *Bismuth in the Absolute Alcohol* (13627).

The Alcohol was poured in to the glass cell and the compensator of 6·4 gr (13627) put on. Three results were:

First time 5ʳ 350°
Again, 6ʳ 0°
Again, 5ʳ 340°
} average 2150°.

13637. *Bismuth and water*. The bismuth was raised carefully without displacing its support or adjustments at all–put on one side–the Alcohol tube removed–cleared out–filled with water and replaced–and the bismuth, which had remained in distilled water during the time, was also replaced, so that all was as before in respect of position. The compensator weight of 7·86 grains (13627) was put into place.

First time 5ʳ 270°
Again, 5ʳ 265°.

When at this extreme (which had been waited for about 15′),

I left the apparatus with the full torsion on, and returning in about half an hour, I found the set had been such as to require 50° more of torsion—which must be taken into consideration.

13638. Made a third observation and obtained 5^r 320°, but as this started from the last extreme and included the 50° of set, it is evident that upon reversing the torsion that should not be counted; so that the observation became 5^r 270°, and I believe that to be near the truth as compared to the first results.

13639. The average of the three is 2068°.

13640. It is clear that when apparatus is set aside, the torsion ought to be taken off. Also that on bringing torsion up to an extreme, the set belonging to 15′ or some given time should be taken for the record. It seems pretty regular as to its repetition.

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13641. Resumed experiments with the bismuth (13592)—is still held by the same suspension, Silk (13595). Arranged its stand so that when once in place it was not changed all day long. The bismuth therefore went always into the same place. It was always in the same polar position between the Magnetic poles, for the indicating wire end above (13608) was always retained on the same side in respect of the stops. The magnetic poles were retained unaltered all the day, except once for the cell (13632) being put into its place; the tubes were changed in it without its displacement and it formed both the block for the pole pieces and the guage of their distance.

13642. As torsion set has to be accounted for, all the following experiments were made thus. A medium being selected and the bismuth hanging in it, the torsion nut was turned direct or reverse until the position was at the extreme. Then 10 minutes were allowed to elapse, that the set due to that time should come on—when the additional amount of torsion was put on and the place of the index, indicated by a like index on the table, moved to the same position, and a note made of the part of the scale it stood at. Then *reverse* motion of the torsion index was put on for 4, 5 or 6 whole revolutions, so as to bring the bismuth into extreme position on the opposite side, and this being left 10 minutes and then the counter set compensated for by further torsion. The index

was then read off and the whole amount from extreme on one side to extreme on the other set down as the result.

13643. For a second observation—the index as just left was recorded; and then 4, 5 or 6 revolutions back were made—up to the other extreme—the whole left for 10 minutes, and then the torsion adjusted and again observed and read off. So that 10 minutes of set is added into each observation at each side. After 10 minutes, the set is but small for the time required in an experiment.

13644. When the medium was to be changed, the torsion was always reduced to Zero—and then with a new medium the process commenced as above (13642).

13645. The bismuth (13592) in pure water. Temperature about 60° F. The compensation weight of 7·86 grains put on (13627). The number of revolution[s] put on at once (13642) was found to be six, and was made so in all the observations.

First time—from extreme to extreme	reverse gave	6^r 55°	2215°;
Again —Do.	direct ,,	6^r 45°	
Again —Do.	reverse ,,	6^r 65°	

then reduced the torsion by 3^r, and so left it for the next expt.

13646. *Bismuth in water* at 60° gave therefore a mean of 2215°.

13647. *Bismuth in Phosphorus* (13618). This phosphorus was very clean and good, and the tube large, being an inch internal diameter. During the whole experiment no bubbles of gas appeared either evolved or adhering to the bismuth. It and the copper appear to have been acted on so far as to be protected from further action—coated either by oxide or phosphuret; and the bismuth has a dark brown colour. The heat was applied to the bath from time to time so as to keep it between 133° and 145° or nearly so. The bath was covered by a copper plate and a card—and the heat and vapour of the spirit lamp was conducted away from the suspension threads above by a spout or chimney applied to the copper bath. The compensation piece of 15 gr. (13627) was put on to the suspender.

First observation gave	4^r 280°	average is 1703° torsion.
Again,	4^r 260°	
Again,	4^r 250°	

13648. *The bismuth in saturated solution proto sulphate of iron* at 60° F. The compensating weight of 9·32 grains put on.

First observation gave	5r 355°	average, 2158° of torsion.
Again,	6r 5°	
Again,	5r 355°	

13649. *Bismuth in Absolute Alcohol* (13627), temp. 60°.

The compensation of 6·4 grains used (13627).

The first observation gave	6r 65°	Average, 2225° of torsion.
Again,	6r 60°	
Again,	6r 70°	

13650. *Bismuth in Air*, temp. 60°. No compensation here required.

The first observation gave	6r 80°	average, 2243° of torsion.
Again,	6r 75°	
Again,	6r 95°	

13651. The results of these experiments on bismuth in different media at one common temperature are as follows:

in Air	2243°	2100°	results 10th March.
„ Alcohol	2225°	2150°	
„ Water	2215°	2068°	
„ Sol. Sul. Iron	2158°		
„ Phosphorus	1703° but temperature must be considered here.		

13652. Now worked with Bismuth in Water—at different temperatures—and first at the temperature of 32°. The division of the copper cell into three parts made this comparitively easy. No glass tube or cell was used, but the water was in the copper vessel and ice pieces in the water in the two outer divisions (13632). The counterpoise of 7·86 grs. was on.

At 32° F.	The first observation gave	6r 150°	Average, 2342° of torsion.
(13669)	Again,	6r 160°	
	Again,	6r 200°	
	Again,	6r 220°	

Above 80° *F.*	result was	5r 245°	temp. varying from	80° to 72°
	Again,	5r 150°	Do.	94° to 87°
	Again,	5r 100°	Do.	102° to 96°
	Again,	4r 310°	Do.	150° to 136°.

13653. So temperature greatly affects the magnecrystallic results.

13654. It was difficult in my experiments to avoid the effects of

currents, and no doubt they did interfere and disturb the observation of the torsion at the extreme position, for small vertical currents, i.e. very slow ones, would topple the bismuth to the one side or the other of the place of instable equilibrium. The lamp was removed from the vessel always 6 or 7 minutes before an observation was made.

13655. I thought I saw signs now and then of an assumption of polar state by the piece, which it gave up suddenly or by a jump when at the point of instable equilibrium, but I think that feeble currents is enough to account for it or them.

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13656. For an account of Thompson's[1] sphere of calc spar and certain experiments with it, see Philosophical Magazine, 1851, I, p. 183 note—and also page 184.

13657. The bismuth prism (13592) when at the same temperature in different media seems to shew but little differential result as to the two axes of power when under the same power of magnet. It will probably shew a small difference dependant upon the better or worse conducting power of the medium surrounding it. Thus a better conductor, as Sul. Iron solution, should have more lines of force passing across it than phosphorus, and so the bismuth crystal shew a greater difference in the first than in the second, because it is in a stronger field of action. In that view also the bulk of the medium around the crystal ought to produce a little effect, for if that were small, being phosphorus, the rest being air, the field ought to be stronger than if all the field were filled with phosphorus. Even the shape of the medium ought to have an effect, for the transfer of power must be more facile

when a diamagnetic of this shape stands equatorially than when it is axial, at least as regards the central spot—and also other parts.

13658. The magnecrystallic actions are easily read and stated by the lines of force representation and Thomson's Magnecrystallic axes.

13659. That the Chief magnecrystallic axis of the body retains the axial position, and with the same amount of force, whether in paramagnetic or diamagnetic media, is perfectly accordant with

[1] William Thomson, afterwards Lord Kelvin.

my view of only one direction of force in the magnetic field, whether occupied by paramagnetic or diamagnetic bodies.

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13660*. I have had a new temperature bath made—it is as follows. It is as if the former cell (13632) of copper were put upon a large reservoir of water, to keep the temperature constant. The dimensions are given. The experimental part between the N and S poles is 1 inch wide by three inches; equatorial direction. The interior is divided into two parts by a vertical plate very near this end and it is intended to pour in hot or cold water at the small mouth *m*, which then passes into the body of the vessel at the bottom *b*, where the inner plate leaves an entrance of a quarter of an inch, and so air, etc. will be prevented from going into the body by the added water. The two cocks are in the chief compartment, for the removal of water above or below. The vessel is to be well cloathed with flannel up to the top and is to be covered by card board over the aperture. Tubes of copper or glass, either open at both ends or closed at the bottom, are to be introduced between N and S, so that currents due to changes of temperature in the body of the fluid shall not affect their contents. It is expected that this arrangement will give a steady temperature for a sufficient length of time. A little oil on the surface of the water in it will much prevent evaporation at that place and loss of heat thereby.

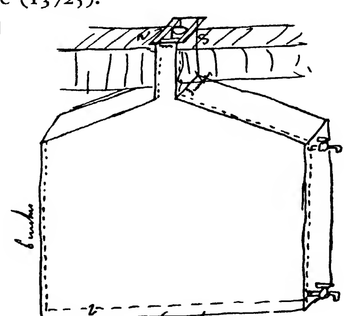
16 MARCH 1855.

13661. Slung eight pieces of bismuth by thread in dilute Nitric acid, near the top—so as to be acted upon and dissected. Some of the surfaces were filled [? filed] and others crystalline, of the same pieces. Two had copper wire in contact. Six were with thread only.

19th March. Not much apparent action—added more N. Acid.

27th March. See (13725).

* [13660]



13662. The great cell has been cloathed well in double flannel. Two vulcanized rubber tubes are attached to the cocks and these come through holes in the flannel, but the cock handles are inside the cloathing and acted on through. These tubes can be prolonged by another tube leading away to a pail on the floor. A pewter funnel introduces the water well at *m*. The thermometer was put into place at the other end of the mouth and then cards over the mouth covered it up when needed.

13663. One flat kettle full of boiling water brought up the whole to 140° F. A second 6′ after brought it up to 180°; it was then full—drew off a quart and refilled with boiling water—was then at 184° F. Had the copper central or sheltering tube in its place. Put a little oil on the surface of the water—and then cards over it and proceeded to observe the fall of temperature by standing.

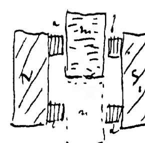
At 1^h 15′		. . 184° F.			
10′			4°	$\frac{10}{4} = 2{\cdot}5$	erroneous first observation, currents, etc.
	25′	. . 180°			
25			15	$\frac{25}{15} = 1{\cdot}7$	
	50′	. . 165°			
5			2	$\frac{5}{2} = 2{\cdot}5$	
	55′	. . 163°			
40			17	$\frac{40}{17} = 2.35$	
	2.35′	. . 146°			
23			9	$\frac{23}{9} = 2{\cdot}5$	
	58′	. . 137°			minutes for each degree of loss by cooling.
77			23	$\frac{77}{23} = 3{\cdot}35$	
	4.15′	. . 114°			
50			10	$\frac{50}{10} = 5$	
	5.5′	. . 104°			
45			7	$\frac{45}{7} = 6{\cdot}4$	
	5.50′	. . 97°			
60			7	$\frac{60}{7} = 8{\cdot}5$	
	6.50′	. . 90°			

So the cloathing is good and sufficient. The water shrank a good deal during the time and will want making up at *m*.

13664. *Torsion*. The cocoon silk is really a good thing—when from the cocoon it has no torsion of its own by more or less

weight. Four or more threads will give all supporting power wanted–the torsion of all comes into play–the set seems steady for a given time–it may be diminished by using longer threads and more of them–for if 6 threads of 6 inches in length take up a certain set by 6 revolutions then 6 threads of 12 inches in length will require 12 revolutions to give the same set, or 12 threads of 12 inches in length will by 6 revolutions give the same torsion force but have only half the set. Those I have used lately seem to have a very constant torsion force for the same weight on different days, and even warmth or a damp atmosphere does not much affect them. When a set of several threads are employed, of course the torsion balance or effect is partly torsion and partly multifilar, but the result is the same.

13665. Suppose N and S too [? two] large magnetic poles, as the pole pieces of the Logeman–*a*, *b*, *c*, *d* four soft iron with helices round them, and then *m* an earthen ware cell filled with freezing mixture and that moved from *m* to *n*. Will the presence or absence of *m* at a low temperature (cloathed in flannel) affect the lines of force across between *a* and *b*–and also *c* and *d*? The four may be connected in one series and with a commutator so that the alternate motion of *m* to and fro shall accumulate the current if there be any produced.



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13666. The *Bismuth* (13592) in *water* at Temperature of 60° F.–working for temperature effects to-day. Used the great copper cell as cloathed in flannel (13660). Had in the copper tube (open at both ends) of 1 inch diameter and 3½ in length surrounding the bismuth. The bismuth height on its support–the pole pieces, etc. being all as on recent occasions. The observations were begun and made as described (13642, 3) except that only 5 minutes was allowed for set to come on at each extreme position.

13667. Made a new air shade round the part of the suspension carrying the counterpoise or compensator (13630), of dark mill board, the white cartridge paper one fatiguing the eye when it was back ground to the stop and parts observed. Sometimes a white and sometimes a dark back ground is required.

13668. The *bismuth crystal* (13592) in water at 60° F.

$8^h\ 15^m$.	The first observation gave (reverse)	$5^r\ 255°$	*Average,*
8.23	Again, (direct)	$5^r\ 180°$	$5^r\ 215°$ or
8.31	Again, (reverse)	$5^r\ 210°$	2015°.

13669. Worked in *water* for *low temperature* (13652). Same bismuth (13592)—the copper chamber well adjusted—the sheltering copper tube in place round the bismuth. The temperature was 33° at the thermometer situated as the bismuth was. There was always ice in the top part of the copper chamber and cell about the bismuth tube.

10h 17m	First observation	(reverse)	6r 5°		
10h 24	Second Do.	(direct)	6r 10°		Average, 6r 35° = 2195°.
10.33	Third Do.	(reverse)	6r 60°		Temperature 34° F.
10.40	Fourth Do.	(direct)	6r 55°		
10.48	Fifth Do.	(reverse)	6r 45°		

Continued to feed with ice during the time at the top. The thermometer at the end was 35° F.

13670. Proceeded to work with the same bismuth (13592) at high temperatures (13652, 13672), i.e. from boiling water downwards. So drew off all the cold water—put in some boiling—left that a while to raise the temperature of the flannel, etc.—drew it off and then filled up with boiling water—placed the sheltering copper tube in its place and the bismuth in it and put a touch of oil only on the upper surfaces, and proceeded to observe. I found a wind current from the window—and had to pull down the sun blind. I found also much current in the water—and these currents were more powerful at the higher temperatures because the cooling then most rapid. Of course, cooling of the internal parts of the mass can only go on by currents, and I rather suspect that the open copper tube is not a protector but may even facilitate vertical currents or spiral currents about the bismuth. Hence the numbers are more uncertain because of the difficulty of observing delicately the extreme point of instable equilibrium.

11h 56m	182° F.	1st reverse	5r 95°	currents troublesome.
12.5	. . .	2nd direct	5r 140°	
12.12	. . .	3rd reverse	5r 90°	
12.19	160° F.	4th direct	5r 60°	water well above the bismuth.
12.33	. . .	5th reverse	5r 80°	more steady.
12.40	. . .	6th direct	5r 100°	

12.46	150° F.	7th reverse	5^r 80°	
12.54	. . .	8th direct	5^r 60°	
1.1	. . .	9th reverse	5^r 135°	
1.8	142° F.	10th direct	5^r 145°	
1.18	141° F.	11th reverse	5^r 100°	water up well—currents.

13671. So the effect of heat in diminishing the magnecrystallic difference evident by comparison with the cold experiments—but the currents in the fluid interfere with accurate observation. Only an average obtainable.

13672. Repeated these experiments in hot and cooling water (13670)—but employed a thin glass tube 1 inch in diameter and closed at the bottom in place of the sheltering copper tube round the bismuth. I also put more oil on the upper surfaces (13670), for evaporation there helps to produce the currents where I want to avoid them. Began to work about 3 o'clk. and went on until 10 o'clk. There were still currents produced but they were less than before and I think all the observations are better. The degrees of temperature are not exact for the graduation is for every 2°, and so I have often left fractions unexpressed. The following are the results, with the time also taken at the close of each observation.

13673. *Bismuth* (13592) in *hot water* gradually cooling.

1	3^h 31^m	reverse	190° F.	5^r 50°	The compensator ( ) not on.
2	3.39	direct		4^r 335°	compensator now on.
3	3.47	reverse	172°	4^r 335°	
4	3.54	direct		4^r 300°	
5	4.1	reverse	169°	4^r 310°	
6	4.8	direct	166°	4^r 312°	
7	4.15	reverse	162°	4^r 332°	
8	4.23	direct	158°	5^r 0°	
9	4.30	reverse	155°	4^r 355°	currents diminishing.
10	4.38	direct	152°	5^r 5°	
11	4.44	reverse	149°	5^r 10°	
12	4.52	direct	146°	4^r 355°	
12[1]	4.59	reverse	144°	5^r 15°	
13	5.5	direct	$141\frac{1}{2}$°	5^r 30°	
14	$5.11\frac{1}{2}$	reverse	138°	5^r 55°	
15	5.18	direct	$134\frac{1}{2}$°	5^r 95°	
16	5.32	reverse	132°	5^r 115°	

The index was then put direct $2\frac{1}{2}$ revolutions, i.e. to zero, and left whilst I was absent. Returning at 8^h 45^m, I found the temperature had fallen to 90° F.

[1] 12 is repeated in the MS.

17	$8^h 57^m$	reverse	90°	5^r 270°	water had sunk in copper part—but not in the glass tube: all right.
18	9.3	direct	89°	5^r 260°	
19	9.10	reverse	88°	5^r 260°	
20	9.18	direct	87°	5^r 280°	

13674. So the diminution of the Magnecrystallic difference by heat very evident.

13675. The resumption of the difference by cooling also very evident.

13676. The magnecrystallic difference disappears probably when the temperature is so high that the diamagnetic force of the bismuth disappears.

13677. If so in all cases, then a *part* of the rate of change in a body might tell us at what temperature its diamagnetic or paramagnetic condition would disappear altogether. Thus, as Calc. Spar sustains a high temperature and is still magnecrystallic, so I suppose I shall find but little change in the relations of the force in two directions by temperatures from 32° to 212°, etc.

13678. But then, all bodies if very hot would loose their diamagnetic or paramagnetic character and become as mere space. This shews that space is or may be a true zero, and is not to be taken simply as a medium, since no paramagnetic or diamagnetic medium can have this relation to other media. Concerns the question between Becquerel and myself whether space is merely intermediate between magnetic bodies, or a true zero between paramagnetic and diamagnetic bodies. Further experiments will elucidate these considerations.

13679. The bismuth was suspended by the silk (13595) continually from $8^h 3^m$ A.M. until $9^h 18^m$ P.M., or during $13\frac{1}{4}$ hours. It worked well all this time and seems but little affected by the continued torsion. In the morng., with water at 60° (13668) the average torsion required for the bismuth was 5^r 215°; in the evening, with water at 87° or thereabout it was 5^r 280 or 5^r 260°, as if its torsion force had diminished a little; but one of the morning observations was as high as 255°. On the other hand, about 1 o'clk. with water at 141° F. (13670) the torsion required was 5^r 100° or more, and at 5 o'clk. or 4 hours after, with water at $141\frac{1}{2}$, the torsion was 5^r 30—as if its torsion force had increased. It is probably but little changed, for the numbers cannot be trusted by themselves.

13680. The bismuth crystal (13592) successively in Air, Water, Alcohol and Sulphate of Iron at the same temperature, 58° F.—and first in Air, the mode of observation being as yesterday. Silk suspender.

13681. The *bismuth crystal* (13592) in *Air* at 58° F.

1	$1^h\,47^m$	reverse	6^r 85°	Average in air at 58° F., 6^r 90° = 2250°.
2	1.54	direct	6^r 90°	
3	2.1	reverse	6^r 100°	
4	2.13	direct	6^r 85°	

13682. The *bismuth crystal* (13592) in *Alcohol of S. G.* . Compensation of 6·4 grains put on (13627). Temperature 58° F. as above.

1	$3^h\,24^m$	reverse	6^r 100°	Average in Alcohol at 58° F., 6^r 109° = 2269°.
2	3.31	direct	6^r 115°	
3	3.38	reverse	6^r 115°	
4	3.45	direct	6^r 105°	

13683. The *bismuth crystal* (13592) in *distilled water*. The compensation of 7·86 grains put on (13627). The temperature 58° F.

1	$4^h\,5^m$	reverse	6^r 75°	Average in Water at 58° F., 6^r 70° = 2230°.
2	4.12	direct	6^r 70°	
3	4.19	reverse	6^r 65°	
4	4.26	direct	6^r 70°	

13684. The bismuth crystal (13592) in saturated solution of proto-sulphate of iron. The compensator of 9·32 grains on (13627). The temperature 58° or 60° F.

1	4.45	reverse	6^r 65°	Average in sol. Sul. iron at 58°, 6^r 74° = 2234°.
2	4.52	direct	6^r 65°	
3	4.58	reverse	6^r 75°	
4	5.4	direct	6^r 90°	

These results very good and especially the two last, which shew that bodies differing as much as water and sol. Sul. iron do not alter the differential quantity of the bismuth axes. The other two are also very near—proving the same thing. In air it is more difficult to obtain the observation because of the tremor of the bismuth.

13685. Now endeavd. to compare Water and phosphorus at the same temperature. So mounted the bath (13660, 13672), put two glass tubes in, one containing the water and the other phosphorus, and raised the temperature of all carefully to above 160°. The

indicating thermometer was between the two tubes—it was easy by shifting the bath a little to bring either tube between the magnetic poles and under the point of suspension. The phosphorus was covered with a layer of water as before (13647). I worked with the bismuth in the water first, and then transferred it quickly to the phosphorus and went on working there—the results were as follows:

13686. The bismuth crystal (13592) in *water*.

1	6h 51m	reverse	155° F.	5r 160°	Average, B. in water 152°·3 F., 5r 145° = 1945°.
2	6.57	direct	153°	5r 140°	
3	7.3	reverse	149°	5r 135°	

13687. The bismuth was now transferred into the *Phosphorus*; compensator changed (13592[1]).

4	7.19	reverse	143°	5r 150°	Average, B. in Phosphorus 138°·7 F., 5r 150° = 1950°.
5	7.25	direct	141°	5r 110°	
6	7.33	reverse	136°	5r 125°	
7	7.38	direct	135	5r 215°	

So here again bismuth is the same both in *water and phosphorus*.

13688. I had some trouble with the bismuth in phosphorus—for a film forms on the phosphorus between it and the water—which clings and interferes with the last degrees of torsion at the instable point; and revolved the bismuth to and fro, but could not permanently break up this film—it was like that on mercury, rapidly opening out if gathered together by a slip of paper and yet stiff when spread over the mercury surface.

13689. The bismuth when taken out had no bubbles upon it. The phosphorus did not stick to it or wet it. It was black on the surface and when swept with wet bibulous paper a loose coat of black particles, small in quantity, came off from its surface—they consisted apparently of phosphuret of bismuth.

13690. *Set of the silk suspender.* In my room carriages make much vibration and this makes the silk set rapidly. So it is not so much the *time* which causes the set to come on as the amount of vibration in a given time. On a stone floor and in the country, it is probable that the set would be very little in a given brief time (13698).

[1] ? 13627.

13691. *Tourmaline*—a piece of black tourmaline cut from a prism by planes perpendicular to the prism—its length is 0·36 of an inch and its diameter 0·37 of an inch. So that it will have little set magnetically except when standing o[b]lique, i.e. with cut faces making an angle of 45° with the magnetic axis. Its weight is 40·4 grains and its Specific Gravity is given as 3·076. Now according to these data and the S. G. of the solutions employed as media (13627), the buoyancy in these media would be as under—and therefore counterpoises of these weights like the former have been made (13627).

13692.

Buoyancy in Air			= 0 grains			
”	*Alcohol*	S.G. 0·816	= 10·1 grains, weight of compensator			
”	Water	S.G. 1	= 13·1	”	”	”
”	Sol. Sul. Iron	S.G. 1·187	= 15·55	”	”	”
”	Phosphorus	S.G. 1·9	= 24·9	”	”	”

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13693. The Tourmaline (13691) was held by a wire just as the bismuth had been (13605), the axis of the prism being horizontal, the side with a flaw in it downwards and the opposite linear angle upwards. Being attached to the silk suspender 5 inches long and 10 fibres, and then placed between the poles up to the cell as before (13660), it took above 40 torsion revolutions to pass from extreme to extreme. I then attached it to a suspender 8 inches long consisting of 50 cocoon fibres and found it required between 13 and 14 torsion revolutions from extreme to extreme. Reduced this suspender (of 50 fibres) to 4·3 inches in length and then it took 6·75 revolutions or thereabouts. This will do.

13694. The Tourmaline (13691) in *Air*. Temperature 57° F., the copper cell bath in place as Guage and so the pole pieces the same distance apart as in the former experiments (13660, 80).

1	$3^h 4^m$	reverse	6^r	345°	Average of Tourmaline in Air at 57° F. is 7^r 14°·3 or 2534°.
2	3.10	direct	7	30°	
3	3.18	reverse	7	25°	
4	3.24	direct	7	10°	
5	3.30	reverse	7	10°	
6	3.36	direct	7	15°	
7	3.42	reverse	7	25°	

13695. The Tourmaline (13691) transferred into Alcohol Absolute of S.G. 0·816 at temperature of 56° F. The compensation of 10·7[1] grains put on (13692).

1	$3^h 46^m$	reverse	7^r 20°	Average, Tourmaline in Alcohol at 56° is 7^r 26° or 2546°.
2	3.62	direct	7^r 35°	
3	4.9	reverse	7^r 25°	
4	4.16	direct	7^r 25°	
5	4.23	reverse	7^r 25°	

13696. The Tourmaline (13691) in distilled Water at 56° F. Compensation of 13·1 gr. on (13692). Had stood at Zero on the Silk $3\frac{1}{2}$ hours.

1	8.6	reverse	6^r	350°	Average, Tourmaline in Water, Temp. 56° is 7^r 21° or 2541°.
2	8.14	direct	7	20°	
3	8.24	reverse	7	30°	
4	8.30	direct	7	33°	
5	8.37	reverse	7	32°	

13697. The tourmaline (13691) in saturated solution of *proto sulphate of iron* at 57° F. The compensation of 15·55 gr. put on (13692).

1	$8^h 55^m$	reverse	7^r	65°	Average of tourmaline in sol. Sul. Iron at 57°, 7^r 112° or 2632°.
2	9.1	direct	7	110°	
3	9.7	reverse	7	107°	
4	9.12	direct	7	122°	
5	9.18	reverse	7	130°	
6	9.24	direct	7	140°	

13698. The *set* of this suspending bundle is important (13690). Consisting of 50 fibres, each has very little weight to bear and the set of each alone is probably very little, but as the whole changes from a twist in one direction to a twist in the other by as much as 7 revolutions, the set of one thread in relation to and against the others may be considerable. Now merely standing for 5′ or more does little or nothing—but a passing carriage does much and a few rapping vibrations with the fingers on the torsion index circle does much. Thus at the end of the last experiment, when the set had acquired a certain amount and the tourmaline was in the extreme position, then the circle was rapped with the fingers; the

[1] ? 10·1 grains (13692).

tourmaline returned a little and required, to bring it to its position

more torsion equal to	45°
rapped it again well—more torsion requd. equal to	25°
Do. Do.	30°
left it for 12 minutes—Do.	20°
rapped it well again—Do.	10°
Change altogether by these rappings	130°

and now seems nearly at an end.

13699. As the vibration from carriages is very uncertain, the whole ought to stand on a stone floor—perhaps in the country—and a certain measured amount of vibration be applied each time—or perhaps none at all, after a first good rapping at Zero.

13700. So as to Air, Alcohol and Water, the Tourmaline may be accepted as sensibly alike in all, for the numbers are respectively 2534°—2546°—2541°—and the average 2540°, the extreme difference being 12° and the results in Air the least and less than the average by 6°. But the difference in iron seems to be a real difference—for the experiments came directly after water—the practise was good and all seemed well. Now the results in the solution of iron are 2632°, or 92° more than the average of the other three, as if the solution, being a better conductor of the magnetic force, had strengthened the power in the magnetic field and that power increase had been made manifest on the bismuth.

13701. With the bismuth however (13651), a result the reverse of this was the case. Must repeat the results—but see (13680–4) or on, (13708).

13702. Remember that bismuth is a diamagnetic body and the tourmaline a magnetic body. Consider what that may have to do with the matter.

13703. Consider also these crystals as measurers of the strength of the forces in the magnetic field, and compare them with the results of a small hard magnet—not subject to change by induction but invariably charged with power. The crystals of course shew no differential results in a field of no force, and less or more difference in fields of less or more force, because of the difference in the power which dominates.

13704. Consider and correct (3158) and any paragraphs having likeness to it. A needle being in a medium as magnetic as itself

would point because of the fixity of direction of the lines of force in it. A needle in a medium much more powerful than it would also point for the same reason—though it would be repelled like a diamagnetic if it could move away.

13705. For former effects of heat on the Magnecrystallic results with bismuth and Antimony, see (2569–2573).

13706. Perhaps a crystal may by change of temperature have one of the magnecrystallic axes pass by another and so a reversion take place: though that does not seem very likely; for—

13707. As yet we know of no case of a paramagnetic body passing into a diamagnetic body by temperature, or vice versa. If the one could happen the other might.

13708. The results of bismuth in Air, Alcohol, Water, and Sol. of Iron not the same yesterday (13680) as they were on the former occasion (13651). Still, in the Solution of iron the bismuth is low (13700).

	Yesterday (13680)	(13651)
Bismuth in—Air	2250°	2243°
Alcohol	2269°	2225°
Water	2230°	2215°
Sol. Sul. Iron	2234°	2158°

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13709. The Tourmaline (13691) at different temperatures in water, and first in ice cold water, as before with the bismuth (13669). The compensation of 13·1 gr. was on (13692).

1	$4^h 34^m$	reverse	34° F.	7^r 355°	Average—Temp. 35° F.— 8^r 4° = 2884°.
2	4.40	direct	34°	7^r 355°	
3	4.45	reverse	35°	7^r 350°	
4	4.50	direct	35°	8^r 2°	
5	4.55	reverse	36°	8^r 17°	
6	5.0	direct	36°	8^r 25°	

13710. This is very different to the former result in water at common temperatures (13696) when the expression was 2541° and the average in Air, Alcohol and water at 57° = 2540—for (13694, etc.)

Tourmaline in Air	at 57° F.	2534	= 2540.
,, ,, Alcohol	Do.	2546	
,, ,, Water	Do.	2541	
,, ,, Sol.Sul.Iron.	Do.	2632	

13711. Now worked with the tourmaline (13691) in water, beginning at a high temperature and observing as before with bismuth as the temperature fell (13673). Same compensator as for cold water on (13692).

1	$5^h 38^m$	reverse	183° F.	8^r 10°	—no sensible signs of currents.
2	5.43	direct	178°	7^r 257°	
3	5.49	reverse	175°	7^r 135°	—appearance of slight currents.
4	5.56	direct	172°	7^r 88°	
5	5.62	reverse	169°	7^r 88°	
6	6.9	direct	166°	7^r 80°	
7	6.15	reverse	163°	7^r 35°	—filled up outer copper vessel with water.
8	6.21	direct	160°	7^r 12°	
9	6.29	reverse	156°	7^r 27°	
10	6.37	direct	153°	7^r 35°	
11	6.43	reverse	150°	7^r 50°	
12	6.50	direct	148°	7^r 50°	
13	6.57	reverse	145°	7^r 98°	
14	7.3	direct	143°	7^r 93°	—filled up copper with water.
15	7.10	reverse	140°	7^r 100°	
16	7.17	direct	138°	7^r 119°	
17	7.23	reverse	136°	7^r 134°	
18	7.30	direct	134°	7^r 140°	
19	7.37	reverse	132°	7^r 125°	
20	7.44	direct	130°	7^r 129°	
21	7.50	reverse	128°	7^r 134°	
22	7.56	direct	127°	7^r 140°	
23	8.3	reverse	125°	7^r 150°	

13712. Now the water of the bath was drawn off—some water at common temperatures introduced—that withdrawn—and then the glass tube surrounding the tourmaline and the bath itself filled with water at common temperatures, i.e. at 62° F. The compensation was put into place and then the four following observations made—

$8^h 34^m$	reverse	62° F.	8^r 70°
8.40	direct	62°	8^r 100°
8.46	reverse	62°	8^r 100°
8.52	direct	$62°\frac{1}{2}$	8^r 90

Average of these four at 62° is 8^r 90° = 2970°.

13713. Again changed the bath, to a still lower temperature, i.e. to 32°, so that the first experiment might be repeated (13709). The same compensator was on all the day. The tourmaline was not cooled otherwise than by being left a while in its appropriate

tube in the bath before the observations commenced. Six successive observations were made.

$9^h\ 21^m$	reverse	34° F.	8^r 198°	
9.27	direct	35°	8^r 228°	Average = 8^r 238°.
9.33	reverse	35°	8^r 245	
9.39	direct	35°	8^r 260	
9.45	reverse	36°	8^r 250	
9.51	direct	36°	8^r 250	

13714. Consider the experiments from a high to a low temperature (13711). The hot results have a much larger differential expression than I anticipated, for I supposed that, as with bismuth, the power would be weak there, whereas it is as strong as in ice cold water, i.e. in the experiments immediately preceeding (13709); and then the power goes on sinking as the temperature falls from 183° to about 160°, and after that gradually rises again as the temperature continues to sink, and it does that through all the rest of the experiments to temp. 125° F. and then to 62° F. (13712), and also still further to 34° F. (13713). So the increase for a falling temperature is evident from 125° F., but what is the cause of a *reverse* effect from 183° to 125°. Either there is a reverse above 125°, or some other cause interferes.

13715. Now in the beginning of the experiment the Tourmaline, previously at 34° F. and then at common temperature, was put into the hot bath. It would be some little time before it rose to the temperature of the bath, at least before the inner part did. Is it possible that whilst heat is going to the inner part, it has a special magnetic condition? So on the other hand, after it had gained throughout a temperature as high as that of the falling medium, then heat would be travelling from the inner part outwards during the rest of the experiments.

13716. So there are several possibilities that require examination. Is there any effect due to the difference of temperature between the inner and the outer part of the tourmaline? Such a difference may act in various ways. Thus when the outside is warmer than the inside, the outside tends to expand, but by its association with the inside, it is then in the equivalent to a compressed state, whilst the inside is in an unpressed or expanded state. On the other hand, when the inside is warmer than the outside (i.e. during the cooling), the inside is in a compressed state and the outside in a

tense expanded state. These conditions would affect a crystal in its action on light. Do they do any thing in respect of the lines of magnetic force?

13717. The Tourmaline when warm is an excellent insulator of electricity. So when raised in temperature, it would become electric, and though the surrounding water would discharge the superficial electricity, or rather by induction compensate it, still the internal parts of the crystal would be electrically polar. This electricity could hardly by any external action affect the position of the crystal, since the water as a conductor envellops it, but it may be the accompanyment of a magnetic effect. Only the difficulty occurs of the reversion of action at temperatures about 160°.

13718. The bismuth shewed nothing of this kind of reversion in the like experiments (13673)—at least I think not, though there is an appearance of it, which must be examined. The bismuth, being a far better conductor of heat and electricity, would not have that difference in the inner and outer parts that the tourmaline might have. Magnetic differences might of course exist.

13719. The common temperature results with the *Tourmaline* this day (13712) are 2970° at 62° F. The common temperature results on a former day (13696) with the same tourmaline, suspender, etc., were very different, being 2541° at 56° F. There is a difference here of 439° [*sic*]. Is this due to a different distance of the pole pieces?—or to a different degree of tremor in the suspension (13698) which to-day has been made considerable by the fingers?—or to a magnetic state acquired by the tourmaline (13717)?—for to-day the tourmaline was descending from a higher to a lower temperature but very slowly only. Perhaps the tourmaline may even take a certain amount of magnecrystallic charge even.

13720. Again, the Tourmaline is very different at the low temperatures of to-day. At first (13709) the average was 2884° at 35° F. Later (about 6 hours after) (13713) the average was 3118° at 35° F., or 234° more, so the increase is still to-day as we progress. What are the causes of this increase? In this case we are proceeding from higher to lower temperature—is that the favouring point? Or is there any thing in the torsion condition; is the silk less rigid or does it set sooner?

13721. Is the gradual increase of the differential amount of the six cold experiments of (13713) any thing indicative? It occurred within half an hour. Does much tapping in an experiment make the observation for future experiments *larger*?

13722. In using the hot water medium—the medium itself is varying in temperature but that ought not to do much in affecting the results, because the Magnecrystallic difference is the *same* in very different media.

13723. On the whole, the results have gone on increasing all day, whether from increased tapping or other causes. It is evidently very unsafe to compare the results of different day[s], or made at different times, together.

13724. To-day at the beginning of the work I made the torsion nothing and then gave a good long vibratory tapping to the torsion index support (13698).

13725. The bismuth pieces in Nitric acid dilute (13661). They are not dissected. The surface seems to shew rather that the solution has been nearly equal in all directions, for the crystalline faces and angles are more indistinct than at first (13661).

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13726. A long experiment with the Tourmaline in water at common temperatures arranged just as before (13696) and with the compensator on. The time comes first in the notes as before; began near Midday.

$12^h\,45^m$—the magnet keeper removed and the bath—tube—tourmaline, etc. in place.
12.50 —The tourmaline hanging—suspender now stretched by the weight.
12.54 —Water into the bath and tube. Temperature 46° F.
1.4 —Torsion on *direct*.
1.9 —index at 70°—torsion *reverse* put on 7^r.
1.14 —after tapping, etc. index at 86°, so

$$\left.\begin{array}{lll} 1.14 & \text{—reverse—}46^\circ\text{ F.} & 7^r\,336^\circ \\ 1.20 & \text{—direct —}46^\circ & 7^r\,349^\circ \\ 1.27 & \text{—reverse—}47^\circ & 7^r\,343^\circ \end{array}\right\}\text{ average } 7^r\,343^\circ.$$

being left with this torsion (reverse) on for 26 minutes, then 115° more required

$1^h\,53^m$—index at 15° from 80°
1.59 —direct —48° F.—8^r 20°

left again under the direct torsion for $2^h\ 17^m$ and then required 208° more to bring it to the direct extreme—much set here.

$3^h\ 22^m$—reverse—53° F.—8^r 8°
3.28 —direct —53° —7^r 320°
3.35 —reverse—53° —7^r 350°
3.41 —direct —53° —7^r 354°

left again under this direct torsion for 53 minutes, at the end of which time it required 111° more of direct torsion. After which at

$4^h\ 40^m$—reverse—54° —8^r 30°
4.46 —direct —54° —7^r 322°

left again with *direct torsion* on for 5 hours and 42 minutes, at the end of which time it required 193° direct torsion more. After which

10^h 33 —reverse—56° F. 8^r 25°
10.40 —direct —56° 7^r 345°
10.47 —reverse—56° 8^r 20°

13727. Being night, it was now left with the reverse torsion on until the morning of the *29th March*, i.e. for 8 hours 40 minutes, and then 392° reverse torsion were required to bring it to the extreme; so much sinking or set having occurred during the night.

$7^h\ 35^m$—direct —54° F.—8^r 12°
7.42 —reverse—54° —7^r 348°
7.48 —direct —54° —8^r 5°
7.54 —reverse—54° —8^r 9°

So the proportional number is pretty constant—though so much one-sided set has been given to the silk.

13728. Now tried the effect of dampness on the silk suspender.

At $8^h\ 0^m$ the reverse torsion (= 4^r about from Zero) was on full; breathed on the silk suspender and immediately 216° more had to be added. Breathed again (three lungs' full) about the silk and now 606° more of torsion were required. On standing 5 minutes and tapping as in the usual mode of observation, 210° more were required. So 2^r 312° or 1032° reverse twist has been thrown into the silk.

13729. Then observations were made of the numbers of degrees requrd. between the extreme positions.

$8^h\ 18^m$—direct—54°—10^r 135°. So the torsion force is exceedingly affected by hygrometric moisture and therefore ever varying. Being left with this direct torsion on for 63 minutes, it required 350° more, and then by a little tapping it requrd. 110° more, or altogether and at once 460°, evidently shewing much flexibility and set. An observation gave at

9^h 30′—reverse—54°—9^r 50°; so the silk is drying and is stiffer

than before—but not nearly what it was before the breathing operation (13728).

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13730. So the varying common temperature results (13719, etc.) easily explicable, and to use silk at all and even for a day it must be enclosed in a glass tube and preserved from hygrometric changes. It is long in recovering its first character.

So also the effect with tourmaline at upper temperature as to increase of torsion is explained (13714), being doubtless due to the action of dampness rising from the bath and affecting the silk (13728, 9). But the effect of temperature upon the Magnecrystallic difference remains only the more distinct and so both bismuth and the tourmaline shew that at the higher temperatures the differential is less than at lower temperatures.

13731. Took other observations as follows:

$12^h\ 55^m$—direct —56° F.—8^r 315
12.61 —reverse—56° —8^r 197
1.8 —direct —56° —8^r 272

So sinking, but is still higher than before breathing on the silk.

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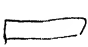
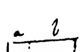
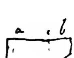
13732. Twelve tourmalines from Mr Tennant, being either colourless or only slightly coloured—to ascertain first if magnetic, and if not, then whether they were magnecrystallic. To ascertain the first they were suspended between pointed poles at the Logeman magnet, and to ascertain the second point flat faced poles were employed.



No. 1. Clear throughout—a fine green colour, of the size drawn[1]. Is magnetic: it points equatorially between the flat poles and is magnecrystallic.



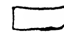
No. 2. Transparent—very little colour—pale green, brown at *a*, faint pink at *b*—cracked a good deal. Magnetic a little—not magnecrystallic sensibly.



No. 3. Pretty clear—blue green at *a*—pink at *b* end—brown between at *c*. A little magnetic—is Magnecrystallic.

[1] Reduced to $\frac{3}{4}$ scale.

No. 4. Colourless–transparent–fine and clear. Very little Magnetic. Not magnecrystallic. When hung with axis vertical, it points feebly with longest diameter axial.



No. 5. Clear–Pink–a good deep tint. Well Magnetic–between flat poles points oblique as if Magnecrystallic, after the fashion of sulphate of iron.

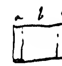
No. 6. Nearly clear and colourless–dirty on the outside, there being black particles in the streaks. Magnetic. Magnecrystallic, oblique as No. 5.

No. 7. Very clear and whole–beautiful–little colour only but pink at end *a*–the rest green blue tint except at *c* where it was nearly colourless. Magnetic. Not Magnecrystallic.

No. 8. Nearly colourless, faint green brown towards one end. Well magnetic. Not Magnecrystallic.

No. 9. Very clear–pink pale–more tint at one end than the other. Magnetic. Not Magnecrystallic.

No. 10. Part at *a* dark, black almost–*b* a pale green brown–*c* colourless. Magnetic, the black end being very magnetic. Was also Magnecrystallic.



No. 11. Clear–colourless–cracked–a little felspar on it in some places. Little magnetic. Not magnecrystallic.

No. 12. A crystal of topaz–pale green tint. A little magnetic–not magnecrystallic.

13733. *Means of Suspension.* Look to Torsion balance (   ). References to suspension of silk, glass, wire, etc. on former occasion, 11086, 7, 8, different substances; 11108, 2036, 55, 390, 578, 619, 37, 42, Platina wire.

13734. Becquerel's wire was silver, the finest possible, being 0·045 of a millimeter or 0·00177 of an inch in diameter. A meter weighed 21 milligrammes, i.e. 100 inches weighed 0·821 of a grain. Ann. de Chimie, xxviii, p. 300.

13735. *My platina wire* (   ). It can hold a weight of 3200 grains–28·5 inches = 1 grain or 100 inches = 3·5 grains. With 23 inches of it as suspender, the tourmaline (13691) required about 200° of torsion from extreme to extreme. Not sensible enough.

13736. I have a *fine silver wire*–that will do I think. 20 inches in length employed as suspender to the tourmaline (13691, 735) required $2\frac{1}{2}$ revolutions nearly of torsion from extreme to extreme.

13737. Examination of Rhumkorff apparatus—Dr. Bence Jones—used from 1 to 10 pair of Grove's plates.

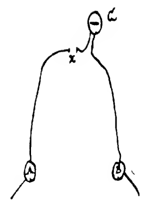
13738. First as to *state of ends simply of the secondary wire*. When the Gold leaf electr. is brought so near as to receive a *spark* from either terminal, it becomes highly charged *positive* by one terminal—*negative* by the other—a change in the direction of the inducing current produces the same effect with reversed character of the charge. A Leyden jar can be charged exactly in the same manner on the same principles—there being always an interval of air between the terminal and the thing charged. The intensity is by the Gold leaf electrometer shewn to be very high. It is easy to obtain single sparks by touching at the batter[y] *after* the apparatus connexions have been completed.

13739. If the Gold leaf electrometer be placed in *permanent contact* with one terminal, a charge the *same in kind* is shewn, but of a very much lower degree. In fact the leaves then shew only the average charge for *continuous contact*. Now when the sparks are allowed to pass most quickly, i.e. between the two terminals and therefore one for every alternation of the breaker, the time during which a spark is passing is as nothing to the time of the interval between it and the next spark—so electrometer charges up for an instant and then falls for a thousand instants (by continual contact with the coil), to rise up for an instant to fall for a thousand again. So the leaves quiver about but retain an average place between the fullest charge and no charge, and that place we see.

13740. The coil as a mass conducts, but the wire in which the current is induced has its coils insulated by silk and varnish one from the other and the coils which are nearest each other are least apart on the *length* of the wire. The coils at the two extremes of the secondary wire, the spark of which is able to break across half an inch of air, would let the spark break through the varnish if they were next to each other—but they have the coils and insulation belonging to the intermediate part of the wire between them. Hence the insulation for a time; i.e. a moment. But the instant

the induction and its spark is over, the wire itself is a *conductor* from end to end and acts in lowering the state of the ends just as the coil conduction as a mass would do.

13741. When the Secondary circuit is made continuous, as by connexion of its terminals, then the spark at the primary breaker is the least; when the secondary circuit is open more or less, there is more or less spark at the primary breaker. Whether the secondary circuit is open or closed makes an important difference which a galvanometer placed in its circuit shews or at least illustrates. So A, B, being the ends of the secondary coil, the wire terminations are carried on to x, a galvanometer being introduced at G. Now when x is closed and all other things remain unchanged, the galvanometer needle moves in one direction by a series of successive impulses coincident with the interruptions at the Primary breaker. It moves, stops, moves, stops, etc., the result being an advance in one direction, which direction shews that the current causing it is due to that induced on *making contact* at the primary breaker. When the ruling contact is made at the V. battery, it is easy to see what the making and breaking does, and then by holding down the hammer breaker so as to keep it in continual contact, then the *making battery contact* gives a strong swing at the Galvanometer–*breaking battery contact* gives a strong swing in the contrary direction–keeping battery contact and letting the hammer make and break, there is the stepping advance of the needle described–the resultant motion being that due to making contact; and when the battery contact is then broken, this intermitting advance is followed by a strong swing in the contrary direction due to the final *breaking* of primary contact.



13742. All this is clear enough, and the reason why in the summation of the results at the Galvanometer a residual action occurs appears to be this. Currents in *alternate* directions due to the *making* and the *breaking* of the primary current run to and fro through the secondary current and galvanometer in it, but each making current is a little stronger than the breaking current. Thus a making current tends to give a strong swing–which is almost instantly stopped and neutralized by the succeeding breaking current, and the reason why the latter cannot quite neutralize the

former is, I think, because a part of the induced current on breaking contact passes in the wire of the primary circuit and is *shewn by the spark* which occurs there. Whatever induction is spent on breaking in that wire is so much subtracted from the secondary wire? This effect is favoured because the induction on making contact is of a lower intensity and takes longer time than that on breaking, and the inducteous is therefore more like to be the equivalent of the inductric force than in the case of the breaking contact, when we know that a part of the induced force passes as a spark in the primary wire and cannot therefore appear directly in the secondary wire (13757).

13743. But *separate the wires* at *x* (13741) in ever so small a degree and then the resultant phenomena at the Galvanometer is *entirely reversed.* On completing battery contact, the needle moves regularly and strongly by a current due to the *breaking* of contact, and ends with an impulse of that kind when the battery contact is broken. This is simple enough. The secondary current being interrupted, those due to making of contact cannot occur in it but are spent in retarding the coming on of current in primary; time is consumed; the tendency to current in the secondary never rises high enough to cross the air interval and the Galvr. is quiescent: but those due to breaking of primary contact are intense enough to cross the air—they pass therefore through the galvanometer and that instrument shews the result in the sum of the effects. The greater the interval at *x*—the higher the intensity required to cross it—the more back action in the secondary wire and the more conduction in the primary wire (shewn by the breaker spark) and therefore the less electricity passing across the interval and through the galvanometer; and the fact is that, though the intensity of the induced current be greater with a large interval at *x* (coincident with action at the breaker), still, the longer the interval the less the quantity of electricity sent through the Galvanometer. The least interval at *x* coincides with the most transfer of Electricity through the secondary coil and Galvanometer, and the deflection then is strongest—as it ought to be.

13744. So the final current in the secondary wire is reversed if the contact at *x* be open or closed. When closed the result is none (13757).

13745. When x is open, the current in the secondary wire is intermitting but all in one direction, and that direction shews it is the induced current due to *breaking primary* contact. This is the current which marks the use and value of the instrument. When quantity with smaller intensity is desired, the interval at x should be small. When intensity is wanted a wider interval there is required. All this agrees perfectly with the Gold leaf Electrometer results.

13746. Proceeded to work with Grove's jar, adding it to the former arrangement. When it is introduced near the terminals of the secondary wire as shewn, then with the same interval at x the discharge there is far more luminous and sounding than when the jar is away.

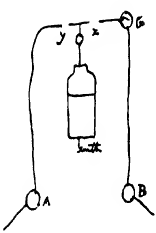
13747. Using now breaking contact effect (for the x interval is always open), there is no more electricity passes than before, for the galvanometer is affected in the same direction and degree as before.

13748. Also if the sparks passing at x are examined in number by a moving mirror, when x interval is small to a certain degree they are found to be more numerous when the jar is away than when it is there, shewing that occasionally the jar charges up; but a different arrangement is required for this effect.

13749. Taking the arrangement as at (13746), the sparks at x accord in their occurrence with the alternations at the primary breaker. Whether the jar be there or not, that is the case, and yet the spark is in one case brilliant and noisy, in the other case dull by comparison and quiet, and each must contain the same amount of electricity. The difference I believe to be due to the *time* of the discharge. When the Jar is there, the induction takes up the Electricity until the whole charge has risen to a certain intensity, and then off it goes at one instantaneous discharge, having only that short and good conductor which connects the outside and inside of the jar, and no part of the secondary coil to travel through then. When the jar is not there, the whole quantity is not discharged at once, for time is concerned; it begins to discharge across x when the induced current has a certain intensity; whilst the progress of the action sustains that current it continues to discharge, and though in one part of that discharge the intensity

is even higher than it ever is when the jar is there (for the induction lowers it), yet the quantity at that instant at the ends of the secondary wire, i.e. near x, is small, and so no powerful light or snap. The great length of the secondary wire aids this effect and the spark is as if a large quantity were by a wet thread diffused over time and rendered a quiet flaming spark. The jar enables this discharge to be accumulated into an instant and hence the change of character.

13750[1]. The sparks are not longer with the jar though they are brighter–they do not contain more electricity than with the dull spark.



13751. The jar was thus arranged, its outside being connected with the earth. Now when both x and y contacts were completed–the feeble current due to making contact was obtained as before (13743, 5); the jar goes for nothing. When either x or y, or both, were open, the galvanometer shewed current due to the breaking of contact of primary as before and the sparks at x and y were both alike and comparitively feeble. The jar still goes for nothing, only there are two interruptions of the secondary current.

13752. But when A y is connected with the outside of the Jar, then the sparks at y are bright and noisy, whilst those at x remain as before. Or if the part connected with the outside of the jar is that from x to B, then the sparks at x are bright and noisy, whilst those at y are quiet, as before. And all this time, whether the sparks at x be noisy or those at y only or neither, the Galvanometer is affected in the same general degree. (Of course if both at once be connected with the outside of the jar, there is a continuous circuit and the sparks at x and y cease and the Galvanometer is affected in the contrary direction).

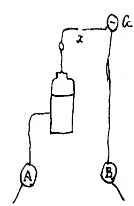
13753. If whilst Ay is connected with the outside of the jar, y be closed up, then its spark disappears–the one at x remains quiet and apparently unchanged, but the galvanometer shews that more electricity is passing through, for the obstruction is less. Then the jar is not in action, but if it be thrown into action by opening y,

[1] Pars. 13750–13805 inclusive were written 23750–23805 and altered afterwards. References in the text were not altered. In transcription, the necessary changes have been made.

then though there is more light and sound, less electricity is going through than when closed.

13754. When one side is connected, the spark on that side is the same as with Grove's jar–the spark on the other side is without that condition–yet both have the same quantity. So this shews the truth of what was said before: the one is a long retarded spark, the other a short quick one. They have their discharges performed in different times. One will probably fire gunpowder, the other not.

13755. But another effect can take place. Each induced current has a given amount of electricity. Now by arranging the size of the jar and the intervals at x and y, and connecting A y with the outside, it is easy to adjust so that two or more sparks shall be required at x to charge the jar up enough to produce a spark at y, and then the sparks at y not only are more sudden than those at x, but contain more electricity; and it is then easy by a moving mirror to see the different number of sparks passing at x and at y–and yet *both* may occur so quickly that the unassisted vision might think that each dull spark at x produced a bright noisy spark at y. When in this way snapping sparks are obtained on one side as at y, the other or x must be considered as the charging side and the noisy place as the discharging side. So also it is better to have the charging side wider than the discharging side, and then the difference in character is more easily seen. The smallest charging spark no doubt occupies more time than the largest discharging spark, for that is instantaneous and has no retardation. The discharging spark is as if by a short thick wire–the other is more like a flaming brush or a water discharge.



13756. When the jar is put into connexion thus, with an interval at x, there is of course no complete secondary circuit. Nor can the induced current circulate. But there is a continual recurrence of sparks at x (and if the jar be a luminous jar, all over the jar itself). The Galvanometer is not affected. The fact is that the discharge oscillates to and fro into and out of the jar–there are double discharges at x in opposite directions–the spark in one direction is instantly succeeded by the reverse spark back again at the same place, and no doubt the passage is prepared by the heat and state into which the first discharge has brought the

space there. This and many other points would connect on beautifully with the perfect dischge. of a jar (Exp. R., 1417, 8).

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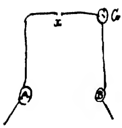


13757. Is there *any* summation of action in one direction when the secondary circuit is *not interrupted*? (13742). Put in the Galvanometer as before (13741). When x is made continuous, there is no doubt movement of the needle in one direction and it swings with intermitting motion and then returns even beyond zero and will go to and fro several times; but when current is stopped at the battery, there is the reverse swing in the other direction due to stopping the current. I believe these two impulses to be equal in amount, only the first is interfered with by the alternations due to making and stopping current by the breaker, and the second, being the last impulse, is left undisturbed by such action. Otherwise there is not any residual action in either direction when the secondary circuit is perfect.

13758. When the circuit at x is interrupted, then only the actions due to breaking contact occur and the motion of the galvanometer is due to the induced currents caused by breaking primary contact (13745).

13759. When the ends at x press together and are separated by interposing the thinnest sheet of paper—a spark occurs and the effects due to the breaking of contact occur; but if the paper be moist and so moist as not to allow a spark, the effect is as if continuity were not destroyed at x and no final deflection of the needle is produced. Paper wetted with brine is as continuity. If ends apart the tenth of an inch and a drop of water be there—the effect is that of continuity. Sixteen thicknesses of damp paper was as continuity. Wet thread was as continuity.

13760. It was only when a spark passed, i.e. when the space x presented a place that the intense charge could pass over, but could not pass in its way back again because of its having been lowered, that a galvanometer current in one direction ensued. When the connecting matter there was such as to conduct both ways, then there was no residual or final result for the galvanometer.

13761. When the wires at x were pressed together with the fingers,

wet paper, etc. acting as continuous conductor being between, there was no shock in the fingers—coincident with no galvanometer current. When the paper was dry and therefore a spark at x, there was shock to the compressing fingers and the accompaniment of galvanometer current.

13762. *Breaker action of the primary coil.* When the secondary coil is out of connexion and action, there is the *bright spark* at the breaker hammer on each breaking of contact, due to Jenkin's effect, i.e. the induction in the primary wire itself. This effect is the same in kind whether 2 or 10 pair of plates be employed.

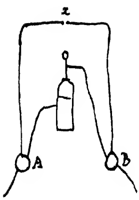
13763. The breaker hammer lifts a little way (then shewing the spark beneath)—falls into contact—lifts again—falls, and so dances or vibrates rapidly, *rarely going up to the core.* Is it stopped in its upward course by any counter magnetism due to counter induction, or is it only because the lift force was very short and therefore very small? I think perhaps it is only the latter and not any momentary *contrary state* produced in the iron core. The little spark and explosion at the breaker surfaces helps in part to blow or lift the hammer up in its rising.

13764. If the secondary current is allowed, the contact at x being perfect, then most of the light, etc. of the induction spark at the breaker is done away with—but still a little is left. There is a very small spark there with the secondary circuit in, and a comparatively large one when it is away. If the distance at x is just within striking distance, then some of the effect at x disappears; and just as x presents less and less interruption, so is the diminution aided, until with perfect contact at x the least inductive spark appears at the breaker. All very consistent.

13765. *Secondary terminals at x.* Their form much influences the length and facility of the spark—points are far better than balls, as they ought to be—both when jars are included in the circuit and when they are away.

13766. The *recurrence* of the discharges at x may be observed by the sound given—by the moving mirror—or moving eye—or by Grove's expedient of the moving paper—the perforations are very beautiful. When the velocity is moderate, they are near together; when great, more open; but no velocity opens the holes to a greater width. When the number was so great as to give a

continuous sound, still very quick motion of the paper would give an interval of two inches between the sparks, shewing how little time is occupied by each spark and how much by the interval between. The interval is easily rendered a hundred times the extent of the perforation, so that the interval must endure at least a hundred times as long as the spark.



13767. *Large jar connected with secondary terminals.* Grove's arrangement. Could put the jar into and out of action quickly. With a certain distance of the ends at x and without the jar, could easily obtain sparks at x with the use of from 3 to 10 pr. Grove's plates on the primary. When the jar *was connected*, could not obtain the sparks at x: the charge went into the jar and returned back again.

13768. Whether the jar was connected or not, when sparks occurred at x they were *coincident* with those at the primary breaker and the *same in number* when the distance at x was such as to allow complete and free action.

13769. Without the Jar (13767) the sparks (using 10 pr. plates) could pass over 0·3 of an inch at x. With the Jar, scarcely over $\frac{1}{30}$ of an inch—as 9 : 1—shewing how much each induced current had been reduced in intensity by its diffusion in the jar, though the quantity of electricity was the same.

13770. A smaller jar gave a less difference—might so go on up to no jar; and thus the jar results resolve themselves.

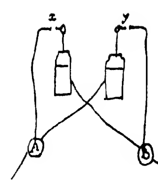
13771. The jar does not increase the quantity of electricity in any one discharge. It does not produce much final result if the secondary discharge be altogether interrupted—except that it diminishes the brilliancy and force of the waste spark at the hammer breaker. The effect is more sensible as the hammer interval is greater and is useful in removing the induction from off the primary wire in some degree.

13772. A large jar lowers the intensity so much as to make it unable to discharge across an interval that could be passed if the smaller jar were used. Hence the advantage of a jar not too large for a given induced current.

13773. The relief which the Jar gives to the spark at the primary breaker (13764, 8) is nothing like so great as that which contact of the secondary circuit at x produces.

13774. Whether the Jar *is there* or *not*, if the secondary circuit be open and out of use, the recurrence of separations and actions at the hammer breaker of the primary circuit is rapid, and the hammer rarely rises up to the iron core, if at all: if the secondary circuit is closed and in use, the recurrences are far less rapid, interval periods occurring and the hammer often lifts to the core. The results are the same with 3 or with 10 pair of plates. So with *no secondary currents* the primary currents are *quicker*, *shorter* and the spark at the breaker seems as large as with the secondary current and fewer intervals. Is there any back action here of the core on the hammer which prevents it from going up to the core? The longer current will make core a more powerful magnet—but why is the current longer when there is the secondary?

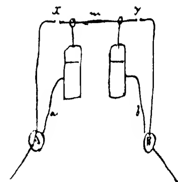
13775. When the secondary is allowed, the primary will rise more rapidly; that in a given time will make the core a stronger magnet; that will lift the hammer more powerfully, and therefore up to it; and that will occupy *more time*, and I think this is the true cause of the effect produced (13774).



13776. Introduced two jars at the terminals of the secondary circuit in two different ways. In this arrangement, if both distances *x*, *y*, were arranged for striking interval, then both gave loud bright sparks under the immediate induction action of the jars. Each spark here must have contained both a to and a fro action I think—examine this again (13788).

13777. If either *x* or *y* opened out beyond limit, then the other gave only the quiet spark; one jar being thrown out of action reduced the case to (13756) and then the spark was the quiet to and fro spark with which there was no final passage (13756).

13778. If either *x* or *y* were closed up to contact, then the other gave a bright loud spark (13791). The jars are first charged by the coil and then discharge each other, so that there should be first a comparitively quiet charging spark and then a loud discharging spark? (13791).



13779. Arranged the two jars thus. The consequences were as follows. If *a* and *b* are not in contact with the insulated jars, then the quiet spark discharge occurs at *x* and *y*; for of course the jars have then no action and there is the simple secondary circuit interrupted at *x* and *y* as in common cases.

13780. If *a* then put in contact with its jar, the *x* discharge either ceases altogether or becomes loud and bright–it ceases altogether if the intensity is so much lowered by the induction as to be unable to dart across there, and then the electricity does not pass through the secondary circuit but merely oscillate there, being the *to* and *fro* spark at *y*; or else the spark becomes loud and bright at *x*, the spark at *y* remaining quiet. In this latter case the condition at (13752) is taken up, for the *y* end and spark make the inside of the jar at *a* acquire one state, the wire and end *a* make the outside acquire the other state, and then the discharge of jar *a* through the short good circuit gives the loud bright discharge.

13781. If *a* be open and *b* be closed up to its jar, the bright spark is at *y* and the quiet one at *x*--all the rest being as before in action, etc.

13782. Bright sparks cannot be obtained at both *x* and *y* at once. If quiet sparks are passing at both *x* and *y*, putting both *a* and *b* in contact with their jars at once stops both the discharges, at *x* and *y*. Then the current in the secondary circuit is stopped and converted into a to and fro oscillation, by *a*, *m*, *b*, and the jars. If the wire *m* be opened in the middle, the to and fro passage is seen there as the soft double spark. The galvanometer shews the same result. The case is like that described before.

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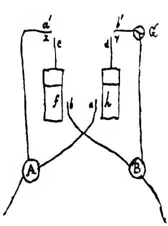


13783. Arranged the apparatus with two equal Leyden jars and the Galvanometer thus–the jars and the Galvanometer being insulated. When *a'* and *b'* were near each other, so as to give the secondary circuit with an interruption there and without induction, then the spark was soft in sound, the Galvanometer was deflected and all was right.

13784. When *a'* was brought near *c*, some very slight sparks were observed there, due to a little induction of the jar *f* across air, etc. When *b'* was near *d* the like effect observed, and when this was done at both places at once, the like effect was observed–the jars being always insulated. The effect is quite natural and due to the intensity change of the ends of the secondary coil.

13785. When the jars *f* and *h* were connected outside with the

earth, the sparks were brighter–the induction through the jars respectively being now more freely allowed–all right.

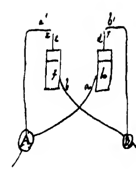
13786. When instead of being connected with the earth they were connected with the ends of the secondary coil opposite to *a′* and *b′*, i.e. jar *f* with *b* and jar *h* with *a*, the effects rose still further–if one jar only was so joined up, the effect there was increased and usually none occurred at the other jar. If both were joined up, sparks occurred at both jars *x* and *y*, and more luminous and noisy than before.

13787. All this time there has been no current through the Galvanometer; the terminals have never discharged to each other–the sparks and actions are due to oscillations to and fro in the secondary wire.

13788. Resumed case (13776)–putting *b* and *f* in contact and *a* and *h* in contact. There were noisy quick sparks at both *x* and *y*–but there was no current through the Galvanometer. So each spark must have been double. The two jars just divided the momentary induction between them and in that respect were as one jar (13756). If one interval was enlarged, then the other jar took up all the action and the sparks appeared at its interval only.

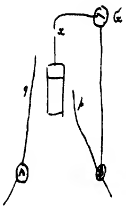
13789. By throwing jar *f* out of action (13777, 13756), there was a quieter spark at *y*, though still a good one, and there was more spark, etc. at the hammer breaker.

13790. To illustrate the influence over the primary circuit, *b* was put in contact with *f*–*a* with *h*–*a′* with *c* and *b′* with *d*; then the induction through the glass of the jars was the freest and but little sparking occurred at the hammer breaker. Opening out *a′ c* threw jar *f* out of action and the spark at the hammer breaker increased: opening out *b′ d* also, by which jar *h* was excluded, caused the spark at the primary hammer breaker to increase to the utmost. In all these cases there was no current through the secondary coil, only alternations.



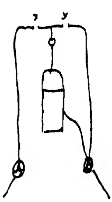
13791. Took up consideration of (13778). If apparatus, being as at 13776 or 13790, both *x* and *y* were open a little, there would be a good spark at both and noisy (13788), but no passage through, only alternating currents. If the *x* space is increased, the *y* spark becomes softer; if the *x* space be diminished, the *y* spark is more and more sudden and the most so when *x* space is closed up by

contact. When the *f* jar is most free to act by induction, it by its connections most quickens the discharge in the *y* space and necessarily so: but all the time there is no current through the secondary circuit, only the alternations.



13792. Of course, when *p* touches the outside of the jar and *q* the inside–there are loud sparks at *x* and a good current through the galvanometer. When *p* touches the inside of the jar and *q* the outside, there is no spark nor current, for A leads to the outside only and B by both wires to the inside, and there is no virtual discharge place or *x*.

13793. The Jenkin's coil effect is connected with these matter[s] and a large jar applied to its terminals ought to diminish the spark there. I tried a small jar, but with little effect; the wire of the helix is not long and is very thick. Probably a battery would take up the intensity charge.



13794. When the jar is connected thus, so as to have a quick loud spark at *y* and a quiet one at *x*, it is beautiful to see the different characters of the sparks, as regards gunpowder or paper there. No continuance of the sparks at *y* through paper will set fire to it, but the same paper is easily fired by the sparks at *x*, though both contain the same quantity of Electricity.

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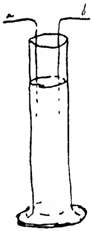


13795. Masson has an experiment on the *Conduction proper of liquids*, of the following kind (See his Prize essay on the E. Spark, p. 87)[1]. A glass vessel contained distilled water. Two platina wires dipped a little way, $\frac{2}{3}$ of an inch, into the water and could be connected at pleasure with the terminals of the secondary coil of a Ruhmkorff's apparatus–the latter being excited by 2, 3, 4 or 5 pr. of Grove's plates.

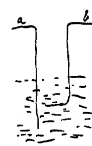
13796. On sending the secondary current through *a*, *b*, gas was gradually evolved on the parts of the wire in the water and more at the   wire than the other. When the current was stopped, some bubbles adhered to the wires–this was instantly displaced by a momentary renewal of current–the current being stopped and all left still, gas appeared gradually on the wires. By renewing

[1] Natuurkundige Verhandelingen van de Hollandsche Maatschappij der Wetenschappen. Tweede Verzameling. Vol. XI, First Part. Haarlem, 1854.

the current, gas apprd. on the wires and not only there but against the sides of the glass at the top part. At the same time the water became warm and I believe all the gas I have seen as yet has been air set free from the warmed water.

13797. Made the circuit at *a* either complete or a little open so as to [ ] a brush spark there, but neither way gave me sensible signs of electrolyzation in the course of 4 or 5 minutes. Yet the water became warm, far warmer than it would have become had a little S.A. been in the tube with it, and far warmer than that dil. acid would have become by a considerable amount of electrolyzation.

13798. *Convective currents.* When the circuit was open either at *a* or *b* about the $\frac{1}{10}$ of an inch more or less up to $\frac{1}{2}$ an inch, the water became charged, and by means of the bubbles of gas, little fibres, etc. strong convective currents were seen in it. When the contact at *a* and *b* was perfect, these were hardly sensible, for then the water had time ( ) to discharge the induced current without displacement. When there was interruption, they were just like the convective currents in non conductors, as camphine, etc. Whichever side was with my apparatus a little open, still the currents always appeared to proceed from the positive wire towards the negative, and not from the negative towards the positive. Even when one wire was bent as in the figure, still whichever was positive, whether *a* or *b*, that sent streams and particles to the other.



13799. These streams were very quick in their motion in different parts, so that the moving particles almost disappeared from the eye. Though there were no striking signs of currents from the N. wire, there must have been a mechanism there by which the portions or particles charged positively could approach, discharge themselves and then move off. All this is well worth examining and referring to its right place amongst the phenomena of convection of liquid insulating bodies (13805).

13800. Must guard the secondary terminal wires with glass tubes or Gutta Percha or some insulating matter.

13801. When a piece of bibulous paper lying upon a glass plate is moistened with sol. iodide potassium, one corner connected by a platina plate with one end of the secondary wire, and a

platina wire connected with the other end of the secondary wire be drawn quickly along the paper, a trace of this kind* is made, shewing the time which each induced current occupies when there is *no interruption* of the circuit—its rise in intensity and its corresponding fall. When an interval occurs, the quantity of action is much less and the places of evolution contract—the intervals of no action being larger.

13802. Whether it be the P. or the N. terminal which is in *contact* with the solution, iodine is evolved there—but if the terminal be lifted above the paper a quarter of an inch or so, then there is a difference, for the mere spark discharge does not then evolve iodine, i.e. the negative wire then evolves no iodine but the positive wire does, acting electrolytically. If when the latter has evolved iodine, the former is brought over the place, the iodine is quickly recombined again.

13803. In the same way, the Negative air discharge over paper moistnd. in solution of Sul. Soda evolves alkali—but the Positive terminal under like conditions does not. It probably evolves acid but I had not the means of trial at hand.

13804. The Negative wire in a solution of Sul. copper deposits copper but the indication is very poor and feeble.

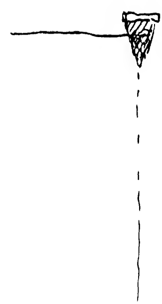
13805. The paper moistnd. with sol. iodide of potassium being on the glass and the Pos. terminal held about $\frac{1}{4}$ of an inch above it at one place, the paper there was soon dried, in part torn up and dispersed—partly upon the Positive wire. The Negative wire had a like action but in an exceedingly small degree. When the Paper was moistened with solution of Sul. soda—the same thing and difference occurred but not nearly in the same degree. When the paper was moistened with distilled water there was scarcely any action of the kind. The difference seemed to depend upon the conductive power. Whether it was the mere action of heat and explosion I do not know.

* [13801]

13806. For further measurements by torsion and that I may employ a metallic wire instead of cocoon silk (13612, 64), so as to be free from the effects of moisture (13728, 9, 30, 1) or set (13690, 8, 9, 726), I have dismissed the torsion apparatus and brought up the former torsion balance (12020) and have been able to combine this with the Logeman Magnet and the use of the hot water bath at pleasure (13663[1]).

13807. The *torsion wire* is *of silver* and very small in diameter–its length is 24·5 inches–it is terminated below by a thin copper plate hook–and is defended in its course by a surrounding copper tube–in two halves.

13808. The *crystal* of bismuth or tourmaline is hung as before (13605, 709) by copper wire–this is thick enough to admit of no sensible torsion–or if it should slightly give way, that will be proportionate to the force applied above by the torsion index and so introduce no error into the results. The length of this wire is 7 inches, and it is surmounted by a flat hook of thin copper plate which hangs on to the former and allows of no twist there. A piece of black bristle is attached to this hook, which serves to govern the position of the crystal by the stops when under torsion. The arrangements are such that the crystal takes up its right place ( ) between the magnetic poles and within the temperature cell.



13809. The *pole pieces* are the same as those used before (13594, 13660).

13810. The *cell* is also the same (13660, 2, 3); it comes up through a hole in the magnet table and is so supported beneath that a lamp can be applied to the bottom and the water either raised in temperature or kept hot.

13811. The *stop* (13608, 32) is a rod of wood passing through one of the stop apertures before described–its end is slit and in this is fixed a piece of card, horizontal–with the edge turned up and cut away in part, so that the two upright edges serve as

[1] ? Par. 13660.

opposite stops on each side of the horizontal bristle attached to the moving crystal (13808).

13812. The *glass plates* of this box balance (   ) serve to cover in the poles, crystal, etc., and shut off all external currents of air.

13813. The *thermometer* (13662), applied in the cell as before, is observed by the use of two mirrors within the box. One throws light on to the stem—which is observed in the other.

13814*. The *cell or bath* (13660, 2, 3) has had a copper ring fixed on to the bottom so that, when raised or blocked into its place, a spirit lamp could be applied there either to raise the heat of the water within or to keep it warm. Every other part of the bath but this is cloathed in double flannel, so as to delay change of temperature; by a temporary screen of copper plate the lamp is prevented from injuring the flannel.

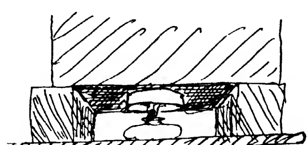
13815. Filled the bath with water—covered the upper surfaces with oil, as before (   )—adjusted the thermometer, etc., having all in working order, and then place[d] an ordinary glass spirit lamp with a moderate flame under the bath, the water being at 67° F.—in 40′ it was 130°—in 2 hours 10′ it was 170°—in 2 h. 20′ at 186°. Because of the position of the screen, the spirit lamp had not air enough at the beginning and the combustion was bad; by adjustment it was better afterwds. and I think less than 2 hours would raise the water up to 190°. At 170° steam appeared on the glass in the box and the inside felt warm. The upper part of the trough cell should be cloathed in cotton wood[1], for there currents occur.

13816. In order to observe the rate of cooling, took away the lamp at 12^h 40′, put on a clean metal cover to the lamp place and left all to cool—the times, etc. were as follows:

At 12^h 40′	186°						
		4°	fall in	10′—	1°	fall in	2′·5
12.50′	182°						
		10°	,,	20′	,,	,,	2
P.M. 1.10′	172°						
		8°	,,	20′	,,	,,	2·5
1.30′	164°						
		16°	,,	43′	,,	,,	2·7
2.13	148°						

[1] ? Wool.

* [13814]



[2.13	148°]			
		14° [fall in]	47′ [−1° fall in]	3·3
3.0	134°			
		11° ,,	45′ ,, ,,	4·1
3.45	123°			
		11° ,,	60′ ,, ,,	5·5
4.45	112°			
		7° ,,	50′ ,, ,,	7·1
5.35	105°			
		19° ,,	205′ ,, ,,	11·8
9.0	86°			
		18° ,,	12 hours	
next morning	68°			

13817. About $\frac{4}{5}$ of the Alcohol in the spirit lamp was employed in raising the temperature.

13818. When the water was at 186° the cell was full to the top; the oil touched the covering card. When it was cold again, the fluid had sunk $1\frac{1}{2}$ inches below the bottom level of the expansion at the top of the trough. I had had to let a little out when temperature about 160°, so that now the level was below the place of the crystal and also of the thermometer bulb. Must watch the expansion and contraction.

13819. I think it will be good to cover the top fairly with oil.

13820. For low temperatures, a small cell will probably be best—and then it should be well covered with cotton, etc. about the top part, or else heating or cooling will make currents there.

13821. Hung up the *tourmaline* (13691, 709) in position to see how all acted, and observed a few preparatory points with it. It remained on the suspension full thirty hours and seemed to be quite right the whole time. The stops, bristle, etc. etc. answered perfectly.

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13822. The *Tourmaline* axis sets equatorial between the poles.

13823. It requires about 2^r 240° or 960° of torsion from the upsetting point on one side to the upsetting point on the other in Air.

13824. The upsetting points are by experiment as nearly as possible 45° on opposite sides of the axial line, which indeed they ought to be. So 90° has to be subtracted from the 960°, i.e. 870° expresses the whole torsion force exerted between one upsetting point and the other.

13825. As the upsetting points ought to occur at the same angle for the same crystal whether its force is greater or less, i.e. however its force may be altered, so the 90° will have to be allowed in every case of this tourmaline, whether it be at high or low temperatures.

13826. Though a crystal of tourmaline or bismuth may be compared with itself, yet portions of different crystals cannot be compared together unless they are alike in size and form, and as respects the torsion wire in weight also–but the latter can be effected by compensating weights.

13827. The bath ought, whilst under observation, to change in temperature very slowly, or else the thermometer and the crystal may not be of the same temperature. Further, as the change can only happen by means of currents in the fluid, if the change is quick the currents are then quick, and the crystal gets a set by them (13564, 5) which introduces error.

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TOURMALINE 13828. The *Tourmaline* (13691) on the torsion balance with wire suspension (13807), *in water* in the bath cell (13660) between the poles (13660). Sweet oil on the top of the water, about $\frac{1}{6}$ of an inch deep. Took of[f] the horizontal axle and its wheel from the torsion plug and turned the latter by hand–the effect was good and the motion easy and even. The ring index was left unaltered, and then the plug turned to the *right*, or with the clock, until the upsetting position on that side was attained, and the degrees of torsion from 0° observed–they were 423°–after a few minutes the index was brought back 10° or 20° and again the upsetting force observed–it was 424°. The observation was repeated in like manner a second time and the result was 427°. Then the torsion plug was turned against the watch, or towards the *left* hand, and observations made in like manner–the results were 620°–632°–636°. Then the right hand observations were repeated and so on several times. The results are below. Temperature 66° F.

Right	423°	424°	427°	average	424°·7	Average right	422°·25
Left	621°	632°	636°	„	629°·7	Average left	628°·85
Right	417°	422°	425°	„	421°·3		1051·1
Left	621°	629°	634°	„	628°		90°
Right	415°	421°	422°	425°	420°·7		
Torsion force between one upsetting point and the other							961°·1

13829. Altered the circular scale, so as to place Zero more nearly in the middle of the whole torsion force—and now obtained the results below: TOURMALINE IN WATER

Right	523°	526°	531°	average	526°·7
Left	515°	525°	528°	„	522°·7
					1049°·4
					90
Torsion force—nearly as before—					959°·4

13830. The successive increases in the series of observations is due to a little set taken up by the fine silver wire—and which no doubt is greater as the time is prolonged or as vibrations are given to the system. To ascertain the amount of this effect in a certain time, when the left hand torsion was extreme, i.e. such as to keep the crystal close to the upsetting point at the first observation, it was then left for 15 minutes, and being then examined, was found to require 13° more of torsion due to set of the wire. Prior to the first observation, the torsion had been to the extreme right, so that the unsetting and resetting were both included. The effect is taken into account by combining left and right hand observations. Temperature of bath now 67°.

13831. The spirit lamp was now applied as before (13814), and the following observations made as the temperature rose. Air was gradually evolved from the water, rising up more and more abundantly till at last the bubbles knocked the crystal about—films also formed between the water and the oil—and the force of the currents was very evident. Observations made as the temperature is rising are not so good as those which are made with falling temperatures. Lamp on at $11^h\ 3'$—temperature then 67° F.

13832. At $11^h\ 14'$ temp. was 84° F.

$$\left.\begin{array}{llllll} \text{right} & 492^\circ & 496^\circ & 497^\circ & \text{average} & 495^\circ \\ \text{left} & 495^\circ & 498^\circ & 498^\circ & \text{,,} & 497^\circ \end{array}\right\} = 992^\circ - 90^\circ = 902^\circ \textit{ torsion.}$$

At $11^h\ 23'$ temp. was 100° F.—so mean temperature was 92° *F. and torsion* 902°. There were bubbles and also currents during these observations.

13833. At $11^h\ 35'$ the temp. was 118°.

$$\left.\begin{array}{lllllll} \text{right} & 458^\circ & 462^\circ & 468^\circ & 468^\circ & \text{average} & 464^\circ \\ \text{left} & 455^\circ & 459^\circ & 456^\circ & & \text{,,} & 456^\circ{\cdot}7 \end{array}\right\} = 920{\cdot}7 - 90 = 830{\cdot}7 \textit{ torsion.}$$

TOURMALINE IN WATER

At 11^h 48′ temp. was 142° F., so mean temp. was 130° *F. and the torsion* 830°·7.

13834. The action irregular from bubbles and currents.

13835. Temp. 156°. Water vapour rises and condenses on the glass–the oil does not remain in a continuous layer–the air and steam bubbles divide it and make open spaces, circular.

13836. At 12^h 5′ temperature was 168° F.; great bubbles rose.

$$\left.\begin{array}{lllll} \text{right} & 414^\circ & 416^\circ & 415^\circ & \text{average } 415^\circ \\ \text{left} & 419^\circ & 422^\circ & 427^\circ & \quad\text{,,}\quad 422^\circ{\cdot}7 \end{array}\right\} = 837{\cdot}7 - 90 = 747^\circ{\cdot}7 \textit{ torsion.}$$

At 12^h 15′ temp. 182° F.–so mean temperature 175° *F. and the torsion* 747°·7.

13837. At 12^h 28′ the bath was simmering and the thermometer (at the top) at 198°. In a little while it was just at boiling. So the lamp was removed and then observations began with a falling temperature–and a comparitively quiet bath–there were no more bubbles and the oil spread itself equably–but currents continued–there could be no cooling without.

13838. At 12^h 35′–temperature 204° F.

$$\left.\begin{array}{lllll} \text{right} & 384^\circ & 387^\circ & 389^\circ & \text{average } 386^\circ{\cdot}7 \\ \text{left} & 404^\circ & 407^\circ & 410^\circ & \quad\text{,,}\quad 407^\circ \end{array}\right\} = 793^\circ{\cdot}7 - 90^\circ = 703^\circ{\cdot}7 \textit{ torsion.}$$

At 12^h 43′ temp. was 199° F. Mean temp. 201°·5 *and the torsion* 703°·7.

13839. No more bubbling. Oil uniform and clear.

13840. At 12^h 50′–temp. 195° F.

$$\left.\begin{array}{lllll} \text{right} & 420^\circ & 425^\circ & 428^\circ & \text{average } 424^\circ{\cdot}3 \\ \text{left} & 418^\circ & 435^\circ & 428^\circ & \quad\text{,,}\quad 427^\circ \end{array}\right\} = 851^\circ{\cdot}3 - 90^\circ = 761^\circ{\cdot}3 \textit{ torsion.}$$

At 1^h 0′ temp. was 188° F.–so mean temperature 191°·5 *F. and the torsion* 761·3.

13841. A little irregularity appeared in the settings of the crystal at this time–as if there were currents–or something else–traced this to another effect–see further on (13852).

13842. At 1^h 10′–temp. 184° F.

$$\left.\begin{array}{lllll} \text{right} & 420^\circ & 420^\circ & 428^\circ & \text{average } 422^\circ{\cdot}7 \\ \text{left} & 421^\circ & 429^\circ & 432^\circ & \quad\text{,,}\quad 427^\circ{\cdot}3 \end{array}\right\} = 850 - 90 = 760^\circ \textit{ torsion.}$$

At 1^h 18′–temp. 180° F.–so mean temperature 182° *and the torsion* 760°. Same irregularity as before.

13843. At $1^h\ 25'$ – temp. 176° F. TOURMALINE

$$\left.\begin{array}{llllll} \text{right} & 432^\circ & 440^\circ & 437^\circ & \text{average} & 436^\circ{\cdot}3 \\ \text{left} & 425^\circ & 430^\circ & 436^\circ & \quad ,, & 430^\circ{\cdot}3 \end{array}\right\} = 866^\circ{\cdot}6 - 90^\circ = 776^\circ{\cdot}6 \textit{ torsion.}$$

At $1^h\ 32'$ – temp. was 172° F. So mean temperature 174° *F. and the torsion* $776^\circ{\cdot}6$.

The water had so far contracted by fall of temperature that I now added a little in the feeding aperture (13660) to fill up and restore the relative level and depth.

13844. At $1^h\ 40'$ – temp. was 169° F.

$$\left.\begin{array}{llllll} \text{right} & 429^\circ & 433^\circ & 440^\circ & \text{average} & 434^\circ \\ \text{left} & 430^\circ & 441^\circ & 443^\circ & \quad ,, & 438^\circ \end{array}\right\} = 872^\circ - 90^\circ = 782^\circ \textit{ torsion.}$$

At $1^h\ 50'$ – temperature 164° F. So mean temperature $166^\circ{\cdot}5$ *and the torsion* 782°. Added a little more water to fill up.

13845. At $2^h\ 5'$ – temp. 158° F.

$$\left.\begin{array}{llllll} \text{right} & 435^\circ & 440^\circ & 446^\circ & \text{average} & 440^\circ{\cdot}3 \\ \text{left} & 450^\circ & 455^\circ & 457^\circ & \quad ,, & 454^\circ \end{array}\right\} = 894{\cdot}3 - 90 = 804^\circ{\cdot}3 \textit{ torsion.}$$

At $2^h\ 15'$ – temp. 154° F. So mean temperature 156° *F. and the torsion* $804^\circ{\cdot}3$.

13846. At $2^h\ 30'$ – temperature was 148°.

$$\left.\begin{array}{llllll} \text{right} & 433^\circ & 438^\circ & 446^\circ & \text{average} & 439^\circ \\ \text{left} & 464 & 468 & 466 & \quad ,, & 466 \end{array}\right\} = 905^\circ - 90^\circ = 815^\circ \textit{ torsion.}$$

At $2^h\ 40'$ – temperature was 145°. So mean temperature $146^\circ{\cdot}5$ *and the torsion* 815°.

13847. At $2^h\ 55'$ – temp. was 141°.

$$\left.\begin{array}{llllll} \text{right} & 446^\circ & 451^\circ & 454^\circ & \text{Average} & 450^\circ{\cdot}3 \\ \text{left} & 461^\circ & 465^\circ & 470^\circ & \quad ,, & 465{\cdot}3 \end{array}\right\} = 915^\circ{\cdot}6 - 90^\circ = 825^\circ{\cdot}6 \textit{ torsion.}$$

At $3^h\ 4'$ – temp. was 138°. So mean temperature $139^\circ{\cdot}5$ *and the torsion* $825^\circ{\cdot}6$. A little more water was added.

13848. At $3^h\ 25'$ – temp. was 132° F.

$$\left.\begin{array}{llllll} \text{right} & 441^\circ & 442^\circ & 443^\circ & \text{average} & 442^\circ \\ \text{left} & 463^\circ & 467^\circ & 471^\circ & \quad ,, & 467^\circ \end{array}\right\} = 909^\circ - 90 = 819^\circ \textit{ torsion.}$$

At $3^h\ 34'$ – temp. 130° F. So average temperature 131° *and the torsion* 819°.

13849. At $3^h\ 46'$ – temp. was 128° F.

$$\left.\begin{array}{llllll} \text{right} & 449^\circ & 455^\circ & 456^\circ & \text{average} & 453^\circ{\cdot}3 \\ \text{left} & 476^\circ & 477^\circ & 485^\circ & \quad ,, & 479^\circ{\cdot}3 \end{array}\right\} = 932^\circ{\cdot}6 - 90^\circ = 842^\circ{\cdot}6 \textit{ torsion.}$$

At $3^h\ 52'$ – temp. was $126^\circ{\cdot}5$. So average temp. $127^\circ{\cdot}2$ *F. and the torsion* $842^\circ{\cdot}6$.

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13850. At $4^h\ 20'$—temp. was 120° F.

$$\left.\begin{array}{lllllll} \text{right} & 468^\circ & 474^\circ & 473^\circ & \text{average} & 471^\circ{\cdot}7 \\ \text{left} & 478^\circ & 481^\circ & 485^\circ & \quad ,, & 481^\circ{\cdot}3 \end{array}\right\} = 953^\circ - 90^\circ = 863^\circ \textit{ torsion.}$$

At $4^h\ 30'$—temp. was 118° F. Average temp. 119° *F. and the torsion* 863.

13851. At 5^h 0—temp. 112° F.

$$\left.\begin{array}{lllllll} \text{right} & 461^\circ & 468^\circ & 473^\circ & \text{average} & 467^\circ{\cdot}3 \\ \text{left} & 485^\circ & 488^\circ & 492^\circ & \quad ,, & 488^\circ{\cdot}3 \end{array}\right\} = 955^\circ{\cdot}6 - 90^\circ = 865^\circ{\cdot}6 \textit{ torsion.}$$

At $5^h\ 10'$—temp. was 111°. So average temp. 111°·5 *and the torsion* 865°·6.

13852. A little water was added here. As the irregularity (13841) still continued, I began to suspect that the tourmaline might in its revolution touch the side of the surrounding tube, for it is not now quite in the middle of the space, the box perhaps having warped a little with the temperature or the moisture; so readjusted it by a slight movement which deranged nothing else and then proceeded to observe.

13853. At $6^h\ 3'$—temp. 103° F.

$$\left.\begin{array}{lllllll} \text{right} & 498^\circ & 498^\circ & 502^\circ & \text{average} & 499^\circ{\cdot}3 \\ \text{left} & 487^\circ & 492^\circ & 496^\circ & \quad ,, & 491^\circ{\cdot}7 \end{array}\right\} = 991^\circ - 90^\circ = 901^\circ \textit{ torsion.}$$

At $6^h\ 25'$—temp. 100° F. So mean temp. 101°·5 *and the torsion* 901°. The observations seem to be much more regular and better.

13854. At $6^h\ 40'$—temp. was 99°.

$$\left.\begin{array}{lllllll} \text{right} & 486^\circ & 491^\circ & 495^\circ & \text{average} & 490^\circ{\cdot}7 \\ \text{left} & 495^\circ & 497^\circ & 499^\circ & \quad ,, & 497^\circ \end{array}\right\} = 987^\circ{\cdot}7 - 90 = 897^\circ{\cdot}7 \textit{ torsion.}$$

At $6^h\ 54'$—temp. was 98° F. So average temp. was 98°·5 *F. and the torsion* 897°·7.

13855. At $6^h\ 55'$—temp. was 98° F.

$$\left.\begin{array}{lllllll} \text{right} & 488^\circ & 494^\circ & 499^\circ & \text{average} & 493^\circ{\cdot}7 \\ \text{left} & 493^\circ & 503^\circ & 503^\circ & \quad ,, & 499^\circ{\cdot}7 \end{array}\right\} = 993^\circ{\cdot}4 - 90^\circ = 903^\circ{\cdot}4 \textit{ torsion.}$$

At $7^h\ 10'$—temp. 97° F. So average temp. 97°·5 *F. and the torsion* 903°·4.

13856. No further observations were made as to the effect of falling temperatures, but in order to obtain a result as to set, the left hand torsion of 503° at $7^h\ 10'$ was left on for 22 minutes—at the end of which time it required 17° more of torsion to bring the tourmaline to the upsetting point; i.e. the set at this amount of torsion was 17° in 22 minutes.

13857. I now cleared off the oil from the surface—then withdrew all the present water and replaced it *by fresh water* at common temperature—leaving the tourmaline, etc. all in order and ready for an observation—and obtained those beneath. TOURMALINE

13858. At 8^h 15′—temp. 72° F.

right	509°	517°	519°		Average 515°	average 514°·3	1044°·3
left	522°	531°	536°		„ 529°·7		− 90
right	505°	517°	519°		„ 513°·7		
left	524°	528°	532°	537°	„ 530°·2	average 530°	954°·3 *torsion.*

At 8^h 35′—temp. 71° F. So mean temp. 71°·5 *and the torsion* 954·3.

13859. So the whole series of observations for the tourmaline runs as under:

rising temperatures	i,	Average temperature	66° F.,	the torsion	961°·1	rising temperatures
	ii,	„ „	67°	„	959°	
	iii,	„ „	92°	„	902°	
	iv,	„ „	130°	„	830°·7	
	v,	„ „	175°	„	747°·7?	
	vi,	„ „	201°·5	„	703°·7?	
falling temperatures	vii,	„ „	191°·5	„	761°·3	falling temperatures
	viii,	„ „	182°	„	760°	
	ix,	„ „	174°	„	776°·6	
	x,	„ „	166°·5	„	782°	
	xi,	„ „	156°	„	804°	
	xii,	„ „	146°·5	„	815°	
	xiii,	„ „	139°·5	„	825°·6	
	xiv,	„ „	131°	„	819° . . .	must be wrong, for it is less than the preceeding No.; is within the irregularity (13852).
	xv,	„ „	127°·2	„	842°·6	
	xvi,	„ „	119°	„	863°	
	xvii,	„ „	111°·5	„	865°·6	
	xviii,	„ „	101°·5	„	901°	
	xix,	„ „	98°·5	„	897°·7	
	xx,	„ „	97°·5	„	903°·4	
	xxi,	„ „	71°·5	„	954°·3	

13860. The torsion force at the beginning and the end accords very well and satisfactorily—the increased magnetic power returns as the temp. falls.

13861. The observations with falling temperatures are better than those with rising temperatures. There is too much agitation of the water in the latter case; at high temperatures, air and steam bubbles interfere and the oil is displaced. Observations v and vi

TOURMALINE are I have no doubt wrong from these causes—perhaps also iv may be liable to the same observation.

13862. Better to make hot observations in Oil itself—and as it cools.

13863. Observation vii is probably doubtful—xiv and xix are also out of order—but on the whole the progression is very satisfactory for a first observation. Had better repeat with this tourmaline in hot water—observing only the falling temperatures.

13864. The thermometer is from its place and nature probably a little warmer than the tourmaline as the temperature rises and a little cooler as it falls; for the cooling must be chiefly from the upper part of the bath.

13865. The difference of torsion force between 66° F. and 191°·5 F. is as nearly as may be 200° of torsion force, i.e. $\frac{961^{\circ}\cdot 1}{200^{\circ}} =$ of the force at 66° F.

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13866. The Tourmaline (13691) just as before (13828) in relation to bath and other arrangements. The protecting copper cylinder was in the water as in former cases (13660).

13867. For low temperatures. Put some ice pieces in the trough cell to cool it and the flannel jacket—filled up with ice cold water—put some pieces of ice on the top in the water outside the protecting cylinder—let all stand awhile so as to obtain a common temperature. Adjusted the moveable circular graduation so as to have the torsion to the right and left nearly equal in respect of Zero. When the temperature, which sank to 34° F. at one time, had become steady, on taking out the few pieces of floating ice, began to observe. In the following columns the time is put first—then temperature—then the terms *right* and *left* to shew the alternate direction of the torsion from zero to the upsetting points—then three observations in succession gained by degree, the *last* of which including the *set* is taken as the right expression for the given temperature—the time is again entered, and then any further needful remark.

13868. As the sum of a *right* and a *left handed torsion* minus 90° give the torsion force between the two upsetting points, so the

last or third results of two consecutive observations, being added together, are taken as expressing the torsion force for the temperature at the close of the first and beginning of the second observation; these results, with that temperature and the correction of 90°, are given in the next three columns. TOURMALINE

		temp. F.	torsion						F. torsion	÷3=
At	8h 26′	38°	right	545	556	*559* at	8h 33′			
							. . . . . .	1112 at 39°	= 1022	340·7
	8.33	39°	left	546	550	*553* „	8.38			
							. . . . . .	1111 „ 40°	= 1021	340·3
	8.39	40°	right	540	553	*558* „	8.47			
							. . . . . .	1109 „ 41°	= 1019	339·6
	8.47	41°	left	546	550	*551* „	8.54			
							. . . . . .	1108 „ 42°	= 1018	339·3
	8.54	42°	right	550	553	*557* „	9.0			
							. . . . . .	1104 „ 43°	= 1014	338
	9.0	43°	left	538	545	*547* „	9.5			
							. . . . . .	1099 „ 44°	= 1009	336·3
	9.5	44°	right	544	550	*552* „	9.10			
							. . . . . .	1095 „ 45°	= 1005	335
	9.10	45°	left	537	541	*543* „	9.14			
							. . . . . .	1092 „ 46°	= 1002	334
	9.14	46°	right	541	545	*549* „	9.19			
							. . . . . .	1089 „ 47°	= 999	333
	9.19	47°	left	533	536	*540* „	9.24			
							. . . . . .	1089 „ 48°	= 999	333
	9.24	48°	right	540	545	*549* „	9.30			
							. . . . . .	1088 „ 49°	= 998	332·6
	9.30	49°	left	531	537	*539* „	9.36			
							. . . . . .	1084 „ 50°	= 994	331·3
	9.36	50°	right	537	540	*545* „	9.41			
							. . . . . .	1082 „ 51°	= 992	330·6
	9.41	51°	left	532	534	*537* „	9.46			
							. . . . . .	1080 „ 51°·7	= 990	330
	9.46	51°·7	right	534	540	*543* „	9.51			
							. . . . . .	1081 „ 52°·4	= 991	330·3
	9.51	52°·4	left	534	536	*538* „	9.58			

13869. Added about 2 oz. of water at common temperatures at the top—stirred—lowered the level by cock—and left it until .

At	10h 3′	54°	right	534	537	*539* at	10h 8′			
							. . . . . .	1075 at 54°	= 985	328
	10.8	54°	left	529	531	*536* „	10.15 temp. 54°·2			
							. . . . . .	1068 „ 55°	= 978	326

Added more water as before—left until

10.20	56°	right	525	529	*532* „	10.24			
						. . . . . .	1061 „ 56°	= 971	323·6
10.24	56°	left	524	528	*529* „	10.27			
						. . . . . .	1063·5 „ 56°·5	= 973·5	324·5

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Added more water as before–left until

10.32	57°	right	530 534 *535* at	10h 37		÷3 =
				. . . . .	1066 at 57° = 976	325·3
10.37	57°	left	525 524 *531* „	10h 42		
				. . . . .	1056 „ 60°·5 = 966	322

Added (as before) *warm* water–left until

10.49	67°	right	518 521 *524* „	10.52		
				. . . . .	1047 „ 64° = 957	319
10.52	64°	left	517 521 *523* „	10·57 temp. 63°		
				. . . . .	1044 „ 65°·5 = 954	318

13870. The cooling of this water is I conclude chiefly the effect of conduction by the copper–not of currents directly amongst the portions of fluid. More warm water added–left–then at

11.1	69°	right	506 519 *522* at	11h 6′		
				. . . . .	1041 „ 67° = 951	317
11.6	67°	left	512 516 *519* „	11.11		

Warm water added at top again–then at

				. . . . .	1039·5 „ 68° = 949·5	316·5
11h 17′	72°	right	512 517 *520* „	11h 22′		
				. . . . .	1038 at 69° = 948	316
11.22	69°	left	512 515 *518* „	11.27		

Warm water added–left–then at

11.32	88°	right	493 500 *508* „	11.37 temp. 81°		
				. . . . .	1013 „ 79° = 923	307·6
11.37	81°	left	496 503 *505* „	11.42 temp. 77°		

13871. Emptied the bath altogether, and refilled it with warm water from a jug–then left until

11h 50′	88°	right	490 493 *499* „	11.54		
				. . . . .	1002 „ 86° = 912	304
11.54	86°	left	496 499 *503* „	11.58 temp. 85°		

13872. Concluded this set of observations.

13873. Proceeded to prepare for beginning at high temperatures (13861), so as to avoid as much as might be the currents due to heating, which are strong, and also the bubbling (13831). Filled the trough cell with boiling water–the tourmaline being in its place–covered the water with oil about $\frac{1}{6}$ of an inch in depth and put cotton wool round the top of the trough. The temperature was now 172° F., the time 12h 10′. Placed the spirit lamp under the bath to raise the temperature higher, and whilst rising made an observation or two just for general indication.

At	12h 15′	176°	right	413 412	12h 17′ strong currents	823 at 181°·5 = 733	244
	12.17	180°	left	408 411	12.20 temp. 183°, no bubbles		

13874. The currents were very strong, so that no good observation could be made—no bubbles as yet—there was very little vapour from the water through the oil—covered over the top of the trough with card pieces. At $12^h\ 27'$ the temperature was 192° F., all quiet and good as regarded bubbles. The torsion index was placed at 0° for the time, i.e. the wire was left without torsion on it. At $12^h\ 30'$ the temp. was 197° F.—and bubbles began to appear. I drew off a little water so as to keep the oil level within the narrow part. At $12^h\ 36'$ the temp. was 208° and the water below close upon boiling. I withdrew the lamp—immediately bubbles and all cleared away—and having left it 2 or 3 minutes I began to observe. TOURMALINE

13875.

		temp. F.							÷3=
At	$12^h\ 39'$	208°	right	382	*388*		12^h 41 currents		
							. . . . . .	784 at 206°F. = 694	231·3
	12.41	206°	left	395	*396*		12.44 currents		
							. . . . . .	786 ,, 204° = 696	232
	12.44	204°·8	right	392	*390*		12.48 little water in	for shrinking	
							. . . . . .	798 ,, 202° = 708	236
	12.48	202°	left	405	404	*408*	12.52		
							. . . . . .	805 ,, 199° = 715	238·3
	12.52	199°	right	397	393	*397*	12.56 currents		
							. . . . . .		
	12.56	197°	left				water added		
							. . . . . .	801 ,, 196° = 711	237
	12.58	196°	left	403	405	*404*	1.1		
							. . . . . .	811 ,, 194° = 721	240·3
	1.1	194°	right	404	406	*407*	1.4½ currents		
							. . . . . .	814 ,, 192° = 724	241·3
	1.4½	192°	left	406	404	*407*	1.9		
							. . . . . .	819 ,, 188° = 729	243
	1.9	188°	right	417	416	*412*	1.15 temp. 185°		
							. . . . . .	820 ,, 185 = 730	243·3
	1.15	185°	left	403	405	*408*	1.18 warm water	added	
							. . . . . .	826 ,, 184 = 736	245·3
	1.18	184°	right	413	418	*418*	1.22 currents		
	1.22	181°·8					dinner		
							. . . . . .	831 ,, 180° = 741	247
	1.29	178°	left	403	411	*413*	1.33		
							. . . . . .	836 ,, 176°·7 = 746	248·6
	1.33	176°·7	right	418	423	*423*	1.36½		
							. . . . . .	844 ,, 175°·5 = 754	251·3
	1.37	175°·5	left	415	420	*421*	1.43		
							. . . . . .	851 ,, 172° = 761	253·6
	1.43	172°	right	424	427	*430*	1.47		
							. . . . . .	854 ,, 170° = 764	254·6
	1.47	170°	left	416	417·5	*424*	1.51		

Tourmaline at different temperatures.

[At	1.47	170°	left	416	417·5	*424*	1.51]				÷3=
							. . . . . .	853 at 169°		=763	254·3
	1.51	169°	right	421	425	*429*	1.54 temp. 168°				
							. . . . . .	857 ,, 167°		=767	255·6
	1.56	167°	left	421	425	*428*	2.0				
							. . . . . .	862 ,, 165°		=772	257·3
	2h 0′	165°	right	422	427	*434*	2.5 currents currents				
							. . . . . .	863 ,, 162°·4		=773	257·6
	2.6	162°·4	left	418	426	*429*	2.10				
							. . . . . .	864 ,, 161°		=774	258
	2.11	161°	right	424	432	*435*	2.14				
							. . . . . .	863 ,, 159°		=773	257·6
	2.15	159°	left	427	430	*428*	2.20				
							. . . . . .	865 ,, 157°		=775	258·3
	2.20	157°	right	430	434	*437*	2.23				
							. . . . . .	878 ,, 156°		=788	262·6
	2.23	156°	left	434	434	*441*	2.29 currents				
							. . . . . .	883 ,, 154°		=793	264·3
	2.29	154°	right	436	440	*442*	2.36				
							. . . . . .	884 ,, 153°		=794	264·6
	2.34	153°	left	438	439	*442*	2.39				
							. . . . . .	887 ,, 151°		=797	265·6
	2.40	151°	right	439	442	*445*	2.45				
							. . . . . .	885 ,, 149°		=795	265
	2.45	149°	left	437	438	*440*	2.50				
							. . . . . .	885 ,, 147°·4		=795	265
	2.50	147°·4	right	440	440	*445*	2.55 temp. 146°				
							. . . . . .	884 ,, 145°		=794[1]	

13876.

at 141° F. *891·5 = 801·5*

3.0	144°	left	433	439	*439*	3.5				
						. . . . . .	890 ,, 143°		=800	266·6
3.5	143°	right	447	447	*451*	3.9				
						. . . . . .	893 ,, 141°·8		=803	267·6
3.9	141°·8	left	441	440	*442*	3.13				
						. . . . . .	894 ,, 140°		=804	268
3.13	140°	right	449	451	*451*	3.17½ temp. 139° filled up with water .	893 ,, 138°		=803[1]	

13877.

at 134°·2 F. *899·5 = 809·5*

3.25	137°	left	437	438	*443*	3.28				
						. . . . . .	896 ,, 136°		=806	268·6
3.28	136°	right	443	451	*453*	3.32				
						. . . . . .	900 ,, 135		=810	270
3.32	135°	left	438	445	*447*	3.37				
						. . . . . .	903 ,, 133°·8		=813	271
3.37	133°·8	right	446	456	*456*	3.41 temp. 132°·5				
						. . . . . .	901 ,, 132°		=811[1]	

13878.

at 129°·1 F. *909 = 899*

3.48	131°·5	left	435	442	445	3.51				
						. . . . . .	907 ,, 130°·5		=817	272·3
3.51	130°·5	right	455	458	462	3.55				
						. . . . . .	913 ,, 129°·8		=823	274·3
3.55	129°·8	left	439	449	451	3.59 currents				
						. . . . . .	911 ,, 128°·8		=821	273·6
3.59	128°·8	right	447	455	460	4.3 temp. 127°·8				
						. . . . . .	911 ,, 127°		=821[1]	

[1] This line is struck out in pencil.

Tourmaline at different temperatures.

13879.

at 124°·1 F. *918·5 = 828·5*	4.10	126°	left	445	449	*451*	4.13		÷ 3 =
							. . . . . .	915 at 125°·2 = 825	275
	4.13	125°·2	right	451	461	*464*	4.17		
							. . . . . .	918 ,, 124°·5 = 828	276
	4.17	124°·5	left	445	451	*454*	4.21		
							. . . . . .	922 ,, 123°·5 = 832	277·6
	4.21	123°·5	right	455	462	*468*	4.25 temp. 123°		
							. . . . . .	925 ,, 121°·3 = 835[1]	

13880.

at 117°·6 F. *933·5 = 843·5*	4.43	119°·5	left	449	442	*457*	4.46		
							. . . . . .	929 ,, 119° = 839	279·6
	4.46	119°	right	463	466	*472*	4.50		
							. . . . . .	932 ,, 118° = 842	280·6
	4.51	118°	left	456	457	*460*	4.55		
							. . . . . .	938 ,, 117° = 848	282·6
	4.55	117°	right	470	476	*478*	4.59½ temp. 116°·2		
							. . . . . .	940 ,, 115°·6 = 850[1]	

13881.

at 113° F. *944 = 854*	5.13	115°	left	457	456	*462*	5.16		
							. . . . . .	942 ,, 114° = 852	284
	5.16	114°	right	478	479	*480*	5.20		
							. . . . . .	945 ,, 113° = 855	285
	5.20	113°	left	460	462	*465*	5.25		
							. . . . . .	946 ,, 112° = 856	285·3
	5.25	112°	right	471	474	*481*	5.28 temp. 112° F.		
							. . . . . .	943 ,, 110° = 853[1]	

13882.

at temp. 108·4 *946 = 856*	5.36	110°	left	456	460	*462*	5.40		
							. . . . . .	944 ,, 109°·2 = 854	284·6
	5.40	109°	right	476	480	*482*	5.44		
							. . . . . .	948 ,, 109° = 858	286
	5.44	109°	left	458	460	*466*	5.48		
							. . . . . .	948 ,, 108° = 858	286
	5.48	108°	right	477	480	*482*	5.52 temp. 107°·7 F.		
							. . . . . .	957 ,, 106°·5 = 867[1]	

13883.

at temp. 104·3 *955·5 = 865·5*	6.3	105°	left	459	462	*465*	6.7		
							. . . . . .	955 ,, 104°·7 = 865	288·3
	6.7	104°·8	right	477	487	*490*	6.10		
							. . . . . .	959 ,, 104°·7 = 869	289·6
	6.10	104°·7	left	460	462	*469*	6.13		
							. . . . . .	956 ,, 104° = 866	288·6
	6.13	104°	right	477	485	*487*	6.17 temp. 103°·8 F.		
								955 ,, 102° = 865[1]	

13884.

at temp. 101° *960 = 870*	6.29	102°	left	459	462	*468*	6.33		
							. . . . . .	958 ,, 102° = 868	289·3
	6.33	102°	right	480	488	*490*	6.36		
							. . . . . .	962 ,, 101°·5 = 872	290·6
	6.36	101°·5	left	461	467	*472*	6.39		
							. . . . . .	963 ,, 101° = 873	291
	6.39	101°	right	475	486	*491*	6.43 temp. 100°·6 F.		

[1] This line is struck out in pencil.

13885.

at 96°·5 F. 974·5 = 884·5

7.6	97°	left	460	463	*467*	7.10			÷ 3 =
						. . . . . .	969 at 97°	= 879	293
7.01		right	485	496	*502*	7.12			
						. . . . . .	977 „ 96°·5	= 887	295·6
		left	461	474	*475*	7.16			
						. . . . . .	980 „ 96°	= 890	296·6
		right	493	502	*505*	7.19 temp. 96° F.			

13886.

7.30	95°	left	460	468	*475*	7.34			
						. . . . . .	981 „ 94°·5	= 891	297
		right	495	500	*506*	7.37 temp. 94° F.			

13887. The low temperature results (13868) are here added on to the series to supply the continuous results—they join on in an exceedingly satisfactory manner and give a fine series.

[F.]	[Torsion]	[÷ 3]	[F.]	[Torsion]	[÷ 3]
At 86°	912	304	At 51°·7	990	330
79°	923	307·6	51°	992	330·6
69°	948	316	50°	994	331·3
68°	949°·5	316·5	49°	998	332·6
67°	951	317	48°	999	333
65°·5	954	318	47°	999	333
64°	957	319	46°	1002	334
60°·5	966	322	45°	1005	335
57°	976	325·3	44°	1009	336·3
56°·5	973·5	324·5	43°	1014	338
56°	971	323·6	42°	1018	339·3
55°	978	326	41°	1019	339·6
54°	985	328·3	40°	1021	340·3
52°·4	991	330·3	39°	1022	340·6

13888. The torsion numbers of the 28 July (13859) are all higher than those of to-day, i.e. for the same temperature in the falling series. Perhaps the magnetic poles had been slightly closer. It shews that watchfulness must be used in comparing different series of results obtained with the same body. Must remember also that an imperfection in the conditions of that experiment existed for a while (13841, 52).

[The results for tourmaline (pars. 13866–97, 14054–7 and 14036) were plotted in graphs included in the MS. at this point. They have not been reproduced.]

13889. Of these results it may be observed, that the gradual progression of the numbers is good, both with the rising and the falling temperature, and shews that the magnetic condition of the

tourmaline varies with the temperature in a constant direction, the power increasing with diminishing temperature.

13890. What small variations there are appear to be due chiefly to the currents which exist in the bath fluid and set the crystal either on the one side or the other. Of course, the bath fluid cannot cool except by currents.

13891. As the temperature falls, the thermometer being at the side of the bath against the copper is probably constantly a little lower than the crystal itself, i.e. a little cooler, and the contrary on rising.

13892. Whether the tourmaline rises or falls to common temperature, the force at a given temperature seems to be restored very accurately. The tourmaline returns to its proper state—there is no disturbance of its specific magnetic capacity for a given temperature.

13893. There is every proof that the force of the magnet continues equal for the time—and also that with the precautions of right and left, the torsion force of the wire continues equal, and that set is, in the manner adopted, eliminated, or at least made proportionate for all the cases in one series of observations.

13894. The whole loss of torsion power between 39° F. and 206° F. is 328 degrees, or 0·3116 of the whole force at 39°—equal to 1·964 of torsion for each degree F.

13895. The diminution from 39° to 123°·5 F. is 190°—that from 123°·5 F. to 206° is 138 degrees—so that it would appear the loss of torsion is proportionately less for higher than for lower temperatures.

13896. When every 10° are taken (or nearly), irregularities appear (due to the current actions (   )), but the main result is the same. A great difference occurs at 86° and 97° F.; this is just where the two series of heating and cooling effects meet (13891).

13897. The effect of this difference of temperature is a very remarkable thing—acting as it appears to do in the same direction both for paramagnetic and diamagnetic bodies.

9TH AUG. 1855.

13898. *Bismuth crystal* (13605, 709, 808) at different temperatures. Employed the apparatus as in the last experiments (13828)

Bismuth crystal.

but without the surrounding copper jacket in the water (13660, 868). Ice and water in first as before (13867)—and by 8 o'clk. A.M.:

		temp. F.						F.
at	8h 54′	45°	right	120	126	128		
							. . . . . .	253 at 45° = 148
	9	45°	left	117	117	125	at 9h 12′ Temp. 47° F.	

13899. The floating ice was adhering to the suspending wire—set it loose by a hot wire, and as the temperature was too high, drew off the bath water on to ice and restored it by pouring through a funnel containing ice also. Now:

at	9h 22′	41°	right	128	130	129		
							. . . . . .	260·5 „ 41° = 155·5
	9.25	41°	left	130	131	131·5		
							. . . . . .	261 „ 41° = 156
	9.30	41°	right	129	129·5	129·5		
							. . . . . .	261 „ 42° = 156
	9.35	41°·5	left	125	130	131	9h 40′ temp. 43°	
							. . . . . .	
	10.0	48°	right	128	128·5	128		
							. . . . . .	256 „ 48°·5 = 151
	10.6	48°·5	left	125	127·5	128	10h 11′ temp. 49°	

13900. Whilst the temperature was rising, I ascertained the angle made by the indicating bristle when at the two upsetting points and found it to be larger than a right angle—and equal to 105°. So that is the quantity to be subtracted as a correction above to give the true result of torsion force between the upsetting points.

13901. Drew out about a pint of water—mixed other water with it to raise its temperature 6° or 8°—made it 53°·5 and introduced it at the usual place—which would partially mix it with the colder water within. Thermometer shewed at the first 53°·5 F. at 10h 30′—left—then:

13902.

								torsion
at	10h 38′	56°·5	right	124	125	124·5		
							. . . . . .	249·5 at 56°·9 = 144°·5
	10.43	56°·8	left	120	127	125	10h 48′ temp. 56·9	
	10.54	replaced the water by cistern water of temp. 65°						
	11.2	65°	right	121	123	122·5		
							. . . . . .	247·2 „ 64°·8 = 142°·2
	11.7	64°·8	left	120	124	125	11.15 temp. 64°·8	
							. . . . . .	248 „ 64°·8 = 143°
	11.17	64°·8	right	122	123	123		
							. . . . . .	248 „ 64°·8 = 143
	11.22	64°·8	left	125	124	125	11.27 temp. 64°·8	
	11.35	water warmed put in (13869)—temp. 79°–79°·2						

Bismuth crystal.

	[Temp. F.]						[F.] [torsion]
11.39	79°	right	120	121	*122·5*	currents	
						. . . . . .	243·5 [at] 79° = 138·5
11.43	79°	left	120	121	*121*	11h 47′ currents 78°·5	
11.56	warm water again—at temp. 90°						
11.58	90°	right	119	120	*120·2*		
						. . . . . .	239·2 ,, 90° = 134·2
12.1	90°	left	114	119	*119*	12.4 temp. 89°·7 currents	
12.12	warm water again at temp. 97°–97°·8						
12.15	97°·5	right	120	122	*120·4*	12·21 temp. 96°·8 currents	239·4 at 97°? = 134·4

[The results for bismuth crystal (13898 *et seq.*, 14036, 14046, 7), bismuth compressed (14045), carb. iron crystal (14039, 40, 14068–70), carb. iron bar (14170–4), iron (14089–90), nickel (14097) and cobalt (14092, 14112) were plotted in graphs included in the MS. at this point. They have not been reproduced.]

13903. *Bismuth crystal.* Now withdrew the water—and introduced boiling water (13873) and covered it as before with oil. The temp. was then at $12^h\,32'$ at 178° F. Put the spirit lamp under—there were strong currents whilst heating, disturbing the crystal much. These currents might affect the crystal of bismuth more because of the absence of the copper cylinder—but they would also have more power than in the case of the tourmaline because of the weak directive force of the bismuth.

13904.

12h 57′	207°	right	100	*102*	great currents	
					. . ↑ . .	200 at 207° = 95
12.59	206°·5	left	100	*98*	Do.	
					. . . . . .	199·2 ,, 206°·5 = 84·2
1.1	205°	right	101	*101·2*		
					. . . . . .	199·2 ,, 205° = 84·2
1.3	203°·6	left	100	*98*		
					. . currents . .	198·5 ,, 203°·6 = 83·5
1.6	202°	right	100	*100·5*		
					. . . . . .	200·5 ,, 202° = 95·5
1.9	200°·5	left	100	*100*		
					. . . . . .	200·2 ,, 200°·3 = 95·2
1.11	199°	right	100	*100·2*		
					. . . . . .	199·2 ,, 199° = 94·2
1.14	198°	left	100	*99*		
					. . . . . .	199·3 ,, 198° = 94·3
1.16	196°·3	right	100	*100·3*	↓	

13905. There is more disturbance at the right upsetting point than at the left one—as if a prevailing current in the cell trough had more influence on the crystal in one position than in the other—perhaps from its form, etc.

13906.

Bismuth crystal.

	temp.			
1h 19′	194°·2	left	100	*100*
1.21	193°·2	right	100	*100·5*
1.24	192°	left	100	*101*
1.27	190°	right	100	*101*
1.30	188°·3	left	100	*101*
1.33	187°	right	100	*100·5*
1.36	185°·2	left	100	*101*
1.40	183°·5	right	100	*101*
1.44	181°	left	100	*101*
1.48	179°	right	100	*101·2*
1.52	177°·5	left	101	*102*
1.56	175°·5	right	101	*102*
1.60	173°·5	left	102	*103*
2.5	171°	right	102	*102*
2.10	168°·5	left	103	*104*
2.15	166°·3	right	102	*102·2*
2.20	164°·8	left	103	*106*
2.25	162°	right	102	*103*
2.35	159°	left	104	*107*
2.45	155°	right	104	*107*
2.50	153°·8	left	107	*109*
2.55	152°	right	107	*109*
3.0	150°	left	108	*109*

. . . . . .	200·3 at 196°·3 = 95·3
great currents	
. . . . . .	200·5 ,, 193°·2 = 95·5
. . . . . .	201·5 ,, 192° = 96·5
currents	
. . . . . .	202 ,, 190°·3 = 97
. . . . . .	202 ,, 188°·3 = 97
. . . . . .	201·5 ,, 187° = 96·5
currents	
. . . . . .	201·5 ,, 185°·2 = 96·5
. . . . . .	202 ,, 183°·5 = 97
currents water added	
. . . . . .	202 ,, 181° = 97
. . . . . .	202·2 ,, 179° = 97·2
. . . . . .	203·2 ,, 177°·5 = 98·2
. . . . . .	204 ,, 175°·5 = 99
. . . . . .	205 ,, 173°·5 = 100
currents added water	
. . . . . .	205 ,, 171° = 100
. . . . . .	206 ,, 168°·5 = 101
. . . . . .	206·2 ,, 166°·3 = 101·2
. . . . . .	208·2 ,, 164°·8 = 103·2
very little disturbance—bubble of air on suspension wire	
. . . . . .	209 ,, 162° = 104
. . . . . .	210 ,, 160°·5 = 105
. . . . . .	212 ,, 157° = 107
. . . . . .	216 ,, 153°·8 = 111
. . . . . .	218 ,, 152° = 113
. . . . . .	218 ,, 150° = 113
. . . . . .	219 ,, 148° = 114

13907. In order to increase the amount of cooling for each 5′, the cotton round the top of the bath was removed—but this so much

Bismuth crystal.

increased the currents, and this so disturbed the bismuth, that I replaced it.

13908.

	temp.					
$3^h 5'$	148°	right	109	*110*	. . . . . .	219 at 148° = 114
3.15	144°·5	left	110	*110*	. . . . . .	220 „ 144°·5 = 115
3.20	143°	right	110	*110*	. . . . . .	220 „ 143° = 115
3.25	141°·6	left	110	*110*	. . . . . .	220 „ 141°·6 = 115
3.30	140°	right	110	*110*	. . . . . .	220 „ 140° = 115
3.35	138°·5	left	110	*111*	. . currents continually . .	221 „ 138°·5 = 116
3.40	137°	right	110	*110*	. . . . . .	221 „ 137° = 116
3.45	135°·6	left	110	*111*	. . . . . .	221 „ 135°·6 = 116
3.50	134°	right	111	*111*	. . . . . .	222 „ 134° = 117
3.55	133°	left	111	*112*	. . . . . .	223 „ 133° = 118
4.0	132°	right	111	*112*	. . . . . .	224 „ 132° = 119
4.5	131°	left	112	*113*	. . . . . .	225 „ 131° = 120
4.10	129°·6	right	112	*114*	. . . . . .	227 „ 129°·6 = 122
4.15	128°	left	113	*113*	. . . . . .	227 „ 128° = 122
4.20	127°	right	114	*115*	. . . . . .	228 „ 127° = 123
4.25	126°	left	113	*114*	. . . . . .	229 „ 126° = 124
4.30	124°·7	right	115	*116*	. . . . . .	230 „ 124°·7 = 125
4.35	123°·5	left	114	*114*	. . . . . .	230 „ 123°·5 = 125
4.40	122°	right	116	*118*	. . . . . .	232 „ 122° = 127
4.45	121°	left	114	*114*	. . . . . .	232 „ 121° = 127
4.50	120°	right	118	*120*	. . . . . .	234 „ 120° = 129
4.55	119°	left	114	*114*	. . . . . .	234 „ 119° = 129
5.0	118°	right	120	*122*	. . . . . .	236 „ 118° = 131
5.5	117°	left	114	*114*	. . . . . .	236 „ 117° = 131
5.10	116°	right	122	*121*	. . . . . .	235 „ 116° = 130

Bismuth crystal.

[5.10	116°	right	122	*121*]		
					. . . . . .	235 [at] 114°·7 = 130
5.15	114°·7	left	114	*114*		
					. . . . . .	234 „ 114° = 129
5.20	114°	right	121	*120*		
5.40	111°	left	114	*113*		
					. . . . . .	233 „ 110°·8 = 128
		right	120	*120*	5h 44′ temp. 110°·6	
6.0	107°·5	left	113	*114*		
					. . . . . .	235 „ 107°·3 = 130
		right	120	*121*	6h 5′ temp. 107°	
6.20	104°·7	left	114	*114*		
					. . . . . .	237 „ 104°·4 = 132
		right	121	*123*	6.26 temp. 104°	
6.45	101°·5	left	115	*115*		
					. . . . . .	238 „ 101°·3 = 133
		right	123	*123*	6·50 temp. 101° currents	

13909. It may be remarked here that, as with the tourmaline, the progression of the observations is good—and pretty regular.

13910. That the diamagnetic force, or the Magnecrystallic force, diminishes with higher temperatures.

13911. That whether with Paramagnetic or diamagnetic bodies, heat causes them to approach the Zero of mere space.

13912. That in this respect the Zero so taken has a real character and is not merely a higher or lower degree of paramagnetic force.

13913. That the change is continually progressive and does not change its sign.

13914. That the state or specific condition is restored with the restoration of temperature.

13915. That the observation from point to point, i.e. by doubling, is very good.

13916. That currents are fruitful sources of error and only to be compensated for by numerous observations.

13917. That to cool without currents is impossible.

13918. Objects to which this method of working might be applicable.

13919. *Bismuth crystal* in hot oil—perhaps the rapid currents in the oil might affect it too much.

13920. Bismuth cylinder ▭ in water or oil—use pointed poles.

13921. Nickel cylinder ▭ in hot oil—currents less consequence, for the magnetic force is far stronger.

13922. Cobalt cylinder.

13923. Phosphorus from 32° or even from 0° upwards to melting point.

13924. Iron even—a very small cylinder, as a wire, in a copper sphere or cylinder.

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13925. *Bismuth crystal.* On laying down the observations as a curve (see the diagram[1]) the irregularities from currents appear, but the diminution of force with rising temperatures is very evident. A straight line would seem best to represent this part of the effect, i.e. between 32° and 212°. When so drawn, the torsion force appears as 88 at 212° and 158 at 32°, giving a difference of 70 between these points or nearly one half of the whole force at 32° F.

13926. Perhaps the bismuth is here, i.e. at 212°, etc., approaching its diamagnetic state, and would shew a more rapid increase of power at lower temperatures, so as to give a curved line. It is worth examining—and phosphorus also.

13927. The Tourmaline observations, being set down, evidently give a curve (see the diagram[2]), and part of a large circle represents this portion of the observation[s] pretty well. The whole loss of force between 32° and 212° is 349, or one third of the whole force at 32°, but the difference, as said before (13895), is much greater at low temperatures than at high temperatures—thus the difference between the temperatures of

40° *19*[3] 60° *19* 80° *17* 100° *16* 120° *14·5* 140°

is 52·1 47·2 42·7 38·7 35·5

13 160° *11* 180° *10* 200° *9* *220* *8* *8* *7*%

32·8 30·5 28·6 of torsion force.

13928. Suppose the following media to be neutral, para- and diamagnetic media, cold and hot:

	cold	hot
Diamagnetic medium, as liquid phosphorus	III	VI
Neutral medium, as water	I	IV
Paramagnetic medium, as sol. Iron	II	V

[1] I.e. the graph referred to after par. 13902.

[2] I.e. the graph referred to after par. 13888.

[3] Numbers in italics are in pencil in MS.

A bismuth crystal, B.C. requires the same torsion force between the upsetting points in I, II and III when of the same temperature: when in IV, V and VI, it also requires like torsion force for the same high temperature, but the amount is less than for the low temperature.

13929. The change in B.C. by heat may be due either to the loss of diamagnetic power along the equatorial line or the gain of diamagnetic power along the axial line—but as bismuth loses diamagnetic power by heat, it is probably the equatorial line loses power by the heat—and also the axial line at the same time, but the latter in a smaller ratio.

13930. For if it were the axial line which gained power (13929), then the *whole mass would gain power* probably; whereas if the equatorial line lose power, then probably the *whole mass would lose*—as it is considered to do, by the few experiments yet made.

13931. If compressed bismuth should be found to act as a crystal, then it is very probable (without other experiments) that the whole mass loses. Also if an amorphous or confused bismuth *prism* should be found to lose diamagnetic force by heat, it will almost certainly shew that the second case above (13930) is the true one.

13932. If the hot B.C. is altogether less diamagnetic than before, there ought to be less difference between it and V and more difference between it and VI than before: i.e. it will have *passed from diamagnetic towards paramagnetic*. It is believed from experiments that that happens with amorphous and confused bismuth.

13933. As with the bismuth crystal, so the tourmaline crystal (13691, 828), T.C., continues to require the same upsetting force for the same temperature whether it be immersed in I or in II or in III (13928): but it loses by heat when in IV, V and VI, but loses alike for the three.

13934. Now T.C. is a paramagnetic body and it may thus change by heat, by its equatorial line gaining paramagnetic force or its axial line losing paramagnetic force: it is probable that the latter is the true case and that the equatorial line also loses force but in a less degree. If the contrary were the fact, then the whole mass would increase in paramagnetic force by heat and that is not true.

13935. If the T.C. loses paramagnetic force altogether, then it will approach in character towards VI and depart from V.

13936. Pieces of noncrystalline paramagnetic and diamagnetic substances, as far as we yet know, [are] affected generally like T.C. and B.C., having the same relations to heat and to I, II and III and also to IV, V and VI.

13937. So assuming IV, V and VI to remain unchanged by the heat which affects the bismuth–the bismuth would in these liquids tend (by quality) to pass from VI to V–and the tourmaline from V to VI. They might not pass across the water or IV, but it is easy to imagine a solution between V and IV that the tourmaline would pass over, supposing the solution underwent no change by heat or a change not proportionate to that of the B. or the T.

13938. If no substance can change its state to such a degree as to cross the zero presented by space, i.e. be converted from a paramagnetic to a diamagnetic body as respects that zero, or vice versa, it would seem to mark the magnetic condition of space Zero as a real and peculiar zero–especially as it is approached by the effects of heat on both sides, i.e. in paramagnetic and diamagnetic bodies.

13939. Whether heat in causing bodies to approach this Zero state makes them to pass by each other or not is not known: if they do not, that would be a very striking fact and is a probable one–it would be very important to know the fact.

13940. If one body can pass another, so as to be paramagnetic to it at one time by temperature and diamagnetic to it at another: and yet cannot do so as to mere space, that would be a very important point to know.

13941. It ought to be possible to have a magnecrystal and a medium about it having a force intermediate to those of the two directions of the crystals. Perhaps tourmaline in Sol. Sul. or Mur. iron, or crystal of Redferropruss. potash in solution of itself. Such a crystal ought to be attracted in the one direction and repelled in the other.

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13942. *Time* in relation to magnetic force.

13943. If the necessity of *time* could be experimentally proved for the propagation of *magnetic force*, it would prove physical lines of force and the probable existence of a medium—on the other hand, if there be a medium, and if physical lines of magnetic force exist, *time* is almost certainly concerned.

13944. But the time (if concerned) will most probably be exceedingly short, like that of its relation to light, and so perhaps for ever remain insensible to us. Still, there is the case of time in the transmission of nervous energy and that is comparatively slow—and there is the fact that when soft iron is magnetized by an electric current, it takes time to rise to a maximum; and here the forces themselves are *magnetic*.

13945. There are two or three great difficulties in the way of comparing the times of the propagation of magnetic force over different distances. First the quickness of the action.

13946. Second—the great distance required therefore to make the propagation such as to require sensible time—and with that great distance the rapid diminution of the magnetic action that is to be made sensible.

13947. *Third*—the want of instantaneous indicators of magnetic action, such slow things as ordinary galvanometers being utterly unfit for the purpose because of the great time they require to start their needles and give them a sensible motion.

13948. *Fourth*—the want of a sudden source of magnetic power, for the bringing of a magnet into position either from a distance or by inversion, or the making of an electro magnet, both require too much time to allow of the small differences I want being rendered sensible by them.

13950[1]. But in respect of these points, consider whether experiments on the following principles may be at all likely to be useful. If, considering the reasons before given (13944), there be the least hopes of finding the *time*, these hopes ought to be verified or exhausted. Can that be done thus?

[1] 13949 is omitted in MS.

13951. The objection (13948), being the fourth above, would be strongest if we attempted to measure the time occupied by the transmission of magnetic force across the space between A and B, *bringing a magnet up to* A *from a distance*; for the time required would be considerable, and unless the time of propagation were almost as much, the latter could hardly be distinguished. If instead of such a process, we had a bar magnet at A and *inverted it*—the time of inversion might be made much less than the travelling time in the former case. Still, it is not likely to be *less* than the time of propagation, and if it were not, the latter could hardly be distinguishable in the first instance. The time required to *make* or *invert* an electromagnet might probably be rendered much less than either of the former periods—still it is very sensible, and if that of propagation is not greater, the latter would hardly appear, especially as the electromagnetization is not a sharp but a progressive operation.

13952. The lateral force of an electric current is of the same nature as the magnetic force, and is as I think the same force, and its generation is as rapid as the production of the electric current itself. Now by the combination of the electric spark and the Voltaic battery, we can make that as sudden as light almost, and if we had only means of appreciating the lateral or magnetic action at a distance, we might I think count upon the generation of the force as instantaneous.

13953. Now the distant appreciation belongs to the third objection (13947):—how can we meet this? If the force were strong enough to generate an electric current in a wire at the place B, that generation would be as instantaneous as the generation of the force at A, and then perhaps the *time* of the propagation from A to B, whether through space, air, water or other media, might appear, being separated. But a new difficulty is, how are we to perceive or know without loss of time the generation of the current at B? Common galvanometers would be far too sluggish to serve as indicators in such a case.

1 954. Cannot a galvanometer be made of a magnet so small and fine as to have only a grain, or the tenth or hundredth of a grain, in weight? The magnet will be strong—even if two are used to make an astatic arrangement, I think that the weight of

the whole need not be more than $\frac{1}{10}$ of a grain. Would not such a magnet shake *instantly* by a very small electric current round it? And it is the first shake we desire to know of. The suspension must be fine cocoon silk and long. The wire round it may be either a single coil or a helix. It is worth while making such a galvanometer needle and trying it by ordinary feeble currents (15405).

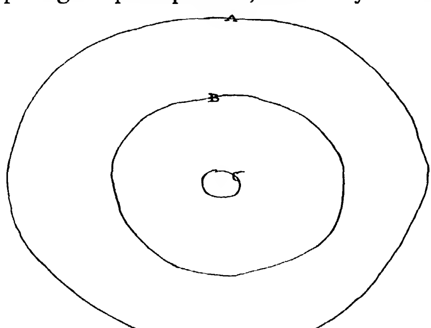
13955*. Now as to arrangements that will include distance:—if A, B, C, be three rings of wire (copper), an electric current set up in one will induce currents in the two others. If a current were sent through A, would not its commencement induce currents in B and C, and if *time* were concerned, would not the current in B occur first, i.e. be separated by an instant from that in C?

The current in C would probably be strong enough because of its central position, though C is more distant from A than B is:—besides, if it were not strong enough, C might be a helix of several coils.

13956. For as the strengthening of the current in C would not make it happen sooner (as far as that depends upon distances), so both B and C might contain each several spirals; but it will probably be best that wire should be added to the circuit of C so that the length in it and its galvanometer should be the same as the length in B and its galvanometer. Probably also B should be close to A, and C in the center far from it—because it is the *time* of the transmission of power between the places of B and C that is the thing sought for. Perhaps A might be a circle 20 or 30 feet in diameter of very thick wire—B a single or double circle of thinner wire 6 inches less in diameter—and C a circle a foot in diameter of the same wire as B and several spirals—but all that would come out by experiment if the principle were applicable.

13957. Supporting the principle true, then many variations of the

* [13955]

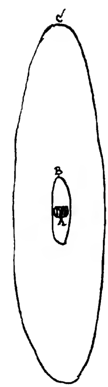


experiments would occur. Thus, if B were half the diameter of A and concentric with it, a short sharp current should be produced in it, for the action of A would be on all parts of B at once. Whereas if B were excentric so that one part was close up to A, the action would be gradual upon it and a longer current of lower intensity would be produced.

13958*. Another arrangement might be to have the three wires parallel to each other in the same line–A being at different distances from B and C. Perhaps more coils in the most distant one would render it sensible enough.

13959. It will be important to ascertain that the time of the commencement of the current has no relation to its strength; i.e. that if C be at one time a single wire and at another time have many convolution[s], that makes no difference on the time as regards propagation of the magnetic force from the source of power A.

13960. Or suppose, as in the next page[1], that A is a short electromagnet, its core being very soft iron, and that B and C are the two inducteous wires; then on making A active by a battery, a commutator might be introduced into the circuit that would change the polarity of A very quickly and so perhaps with a sharpness enough (13951) to give a difference of time evident at B and C.



13961. Or A might be a helix without soft iron.

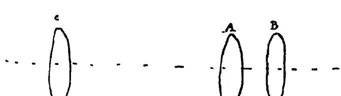
13962. It might be useful to apply such a commutator to the source of power current in the former experiment (13955).

13963. All the apparatus ought to be perfect in action, for the effect sought for must be one soon swallowed up by irregularities or deficiencies of action. It is the first movement of the galvanometers that is to be sought for and not the amount of deflection. They may perhaps require to be close together for more careful comparison in the time. They may perhaps require to be sheltered from the external or acting magnets and currents, and could be so sheltered by iron chambers about them.

13964. If an effect of time should be by any possibility observed, then we should have to proceed to investigate the effect of media introduced into the space traversed by the Magnetic force. For media may differ in several points and from several causes. Some

[1] I.e. in the diagram.

* [13958]



allow the formation of Electric currents in them and some do not. Some are paramagnetic and others diamagnetic.

13965. In regard to the question, are lines of force around a wire carrying an electric current displacable as those issuing from the poles of a magnet are, it may be assumed that they are. But it does not appear that a current has any lines of force about it except those it excites in the surrounding media. A helix excites a certain sphondyloid of power in the air—or in space or in water—or mercury. If iron be in it as a core, more is excited in it than in the air core, but the lines are all closed curves and the sphondyloid contains more power than when air was in place of the iron. Or if bismuth be introduced, the Electric current excites fewer lines of force in it and the whole sphondyloid contains a less amount than in air alone.

13966. Hence the action of the bismuth in the phenomena with Weber's apparatus as shewn to me by Tyndall last Tuesday (the 14th). I shall like to know what kind of likeness and of difference there is between bismuth on the one hand and Phosphorus or heavy glass on the other.

13967. If a prism of amorphous bismuth should give the *same ratio* for change of power at different temperatures as that given by a cube or sphere of bismuth cut out of a crystal, then it will shew that the changes in the force in the axial direction are the same proportionately as the changes in the equatorial direction; i.e. that there is but *one bismuth ratio* and that it is the same for the force in the axial direction of a crystal or the equatorial direction—or for amorphous bismuth.

13968. Only—this should happen in a zero medium alone—for if the bismuth were in a medium equidistant between the axial and equatorial force and not changing by heat with it (13937–41), then heat ought to make the force in both directions less diamagnetic, which, if the medium changed not, would make the axial force more paramagnetic in relation to it than before and the equatorial force less paramagnetic, and the latter would at last pass the medium in character and also become paramagnetic in relation to it.

13969. If substances could thus pass each other (13939), can they also by heat pass the magnetic character of space? If they cannot pass each other, they form a series *constant in order at all temperatures.*

13970. If they can pass each other, and not space—then space alone and space occupied by matter differ.

13971. If they can pass space as well as matter, abundance of new thoughts will arise.

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13972. When a large bath is employed (13660), being well cloathed, almost all the cooling is from the top, and the mass below is cooled by convection currents, which sadly disturb the object under examination. Now a smaller trough cooling more rapidly would have less mass of liquid to move, but the currents would be more rapid and therefore bad. A very small trough exceedingly well cloathed, so as to be as long in cooling as the larger trough, would give the fewest and weakest currents. Is not a *closed* protecting cylinder of copper such a small trough, heated and cooled by a large outer regulating bath? Is not this the right principle—especially with hot baths of oil?

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13973. Have attached various substances to suspenders so as to allow of their being put on to the torsion balance and tried for power between the poles of the Logeman Magnet (13806, 9). The torsion wire is the fine silver wire (13807), the pole pieces are the square masses with flat ends (13594). These were brought up to the sides of a copper cell and were then 1·14 inches apart. Such is the power, the torsion force, etc.—all the trials being made in the air and of a rough character only.

13974. The *sphere of calcareous spar* (13593) which was tried before with a silk suspender (13601, 2), was now hung up, it being held by fine copper wire going over it in four places. The optic axis was horizontal. The upsetting points could scarcely be distinguished, and not more than a few degrees of torsion could be perceived as due to set between them. So the wire will not do—must use the silk (14123).

13975. *Bismuth compressed in one direction.* A piece—a single piece—0·5 of an inch high and 0·36 of an inch square, is suspended so that the line of compression is in the horizontal direction; the suspending copper wire goes once round it in a vertical plane and the axial line of pointing is transverse to this plane. The piece weighs 128·5 grains. The upsetting points were easily distinguished and very good. The whole torsion force between them was about 280°—and abstracting 90° or 100° for angular difference of the upsetting points—we have 180° or 190° for measurement. This is quite sufficient.

13976. *Crystal of Carbonate of iron.* A good rhomboid was selected and the faces *a*, *a*, terminating the optic axis, being converted into planes perpendicular to that axis, the linear angles *b*, *b*, were then replaced by grinding so as to give faces perpendicular to those on *a*, *a*, thus forming a rough quadrangular prism. This prism was made octagonal by removing the solid angles formed by the four artificial planes, and was then suspended at *c* by the fine copper wire going once round it in a vertical and once in a horizontal direction. The height of the crystal from point to point is 0·6 of an inch—the average horizontal width is 0·37 of an inch and the weight 47·5 grains. When in place, the short axis of the crystal (the optic axis), which is in the plane of the vertical suspending loop, sets in the axis of the magnetic field.

13977. The set of this crystal was far too powerful for this suspension wire and these intervals—it would give the wire an unmanageable *set*. So put the pole pieces back until they were as open as the limbs of the magnet, and then the interval was 4·7 inches. The torsion between the upsetting points was now roughly 620°, which minus 90° gives 530° about of force; this is abundantly enough for this wire and specimen.

13978. One might use another and thicker torsion wire, as that probably required for iron, nickel, and cobalt (13990). One might also take off part of the power of the magnet by a cross piece of iron on it; but as the coercitive force of that iron might give way more or less, it would cause irregularity in the amount of magnetic force in the field.

13979. *Red ferro pruss. potassa.* The crystal points best when

the plane passing through the axis of the crystal and the two obtuse linear angles is vertical—that plane takes up the equatorial position, and the line perpendicular to it is that which coincides with the magnetic axis of the field and there is the most paramagnetic line in the crystal. Selected a crystal and ground off the two ends on sand paper until its length was 0·54 of an inch—then removed the two acute linear angles of the piece until the bredth was 0·48 of an inch, suspended it with the obtuse linear angles horizontal and therefore the chief lines of force horizontal (the height 0·46), by the fine copper wire passing once round it in the plane of the chief magnetic line of force. The weight of the crystal was 32·5 gr.

13980. When in place, this crystal pointed very well and gave good upsetting points. The whole motion of the torsion index was 375°, which when minus the angle of upsetting points, will leave plenty for measurement, so that the wire will do.

13981. I turned the pole pieces round (13594, 973), their points being at the same distance as the planes were before. The power was not so much as before, the whole motion of the torsion index being about 250°, or only ⅔rds of the former. This is a good result and ought to be the case. The upsetting points were very clear, but were very far apart, 130° or 140° about: what they will be with the square ends I have not yet ascertained. It will be interesting to note and consider the difference.

13982. Plücker says this salt is decidedly paramagnetic. I and he had said it was diamagnetic, so it must be near to Zero; and being highly magnecrystallic, it ought to be easy to find a medium in which it should be paramagnetic in one direction and diamagnetic in another.

13983. Replaced the pole pieces with flat ends against the measuring copper trough (13973) and proceeded to try heavy glass, amorphous bismuth and phosphorus.

13984. A square *prism of heavy glass* 0·6 of an inch long, 0·26 deep and about 0·12 in thickness was suspended with its length and thickness horizontal by the fine copper wire passing once round it. When in the magnetic field, I was scarcely able to distinguish any upsetting points, so little is the power of the diamagnetic set. Turned the pole pieces round so as to have the points

at the interval, and therefore a variable field of force. Even then, the set not sensible. So this wire will not do and I must use a silk fibre.

13985. *Prism of amorphous* or rather *granular bismuth*—length 0·55 of an inch—depth 0·3—thickness 0·12, suspended by copper wire with the length and thickness vertical. It was more sensible than the heavy glass but not enough; there were only a few degrees of torsion above that of the upsetting angle and this wire will not do with such opening. The pointed poles better than the flat faced poles—the upsetting points were then nearly 180° apart, i.e. reckoned on the same side of the magnetic axis. Of course, they came very nearly up to the plane of the magnetic axis and were a very little way apart across it.

13986. *Prism of phosphorus*—about the same size as the bismuth (13985)—its action not sensible with this torsion wire and intervals.

13986½. Now proceeded to try some very paramagnetic substances, expecting that this wire would be quite unfit to work with them. Put the pole pieces back as far as possible, leaving the large interval between of 4·7 inches (13977).

13987. *Iron.* A small copper cube, 0·25 of an inch in the side, was suspended as the object—a fine hole was made through it in a horizontal direction from the middle of one face to the middle of the opposite. A piece of annealed iron wire, 0·0166 of an inch in diameter and 0·25 in length, was placed in this hole. When in the magnetic field, the set of this cube was such that two revolutions of the torsion index caused scarcely a sensible displacement. Reduced the iron wire to 0·1 in length; even then, three revolutions of the torsion index did not take the cube far from its normal position. Reduced the length of the wire to 0·05 of an inch, and now it requires 3¾ revolutions of the torsion index from one upsetting point to the other: this is too much for this torsion wire.

13988. I took away the pole pieces altogether, which is equivalent to a certain withdrawal of the magnet; and then the torsion was more manageable, requiring two revolutions and 120° from point to point.

13989. One might have a thinner wire, but the question is whether lessening the diameter or shortening the length is the

best expedient to obtain a manageable mass. One has to consider the liability to magnetic charge of the body and in that respect perhaps a short thick piece might be best.

13990. However, probably use the thicker platinum wire for these paramagnetic bodies.

13991. *Nickel.* I have a small disc of the pure metal about the size given[1]—was quite unmanageable, flying to this and that side inevitably. Must have a very small piece and load it with copper.

13992. *Cobalt*—pure. I have a prism about this size[1]—just as the nickel—far too powerful.

13993. In respect of the Galvanometers before thought of for the investigation of *time* (13954), their indication might be made more nearly instantaneous by converting the upper needle into a reflector. Thus suppose the needle *n* a piece of square and polished steel, suspend[ed] as shewn by *s*—and suppose *l* a luminous object sending a ray from the reflecting surface to the eye at *e*. With the least motion of the needle round the axis *s*, the object *l* would pass out of the reflector at one end or the other according to the direction of motion, and the more rapidly as the object *l* is at a greater distance. I think that two needles, starting into motion at differences of time so small as not to be observable by the eye looking at the needles themselves, would easily give a sensible difference by looking at these reflected images. For still greater delicacy, it is possible that Wheatstone's or Foucault's revolving mirrors could be combined with this kind of observation. A distant Voltaic or lime light might be used—or the light of the sun. A round needle would not do, for then different parts of the convexity reflect at different positions, and the image seems only slowly to leave its place because it appears on different parts of the reflecting surface.

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13994. The Carb. iron crystal (13976) varnished with white hard varnish and hung up in the warm air cupboard to dry.

13995. The red ferro pruss. pot. crystal (13979) varnished in like manner and put to dry. Three or four other crystals, with

[1] Reduced to $\frac{3}{4}$ scale.

copper wire holders attached to them, were varnished in like manner and put to dry.

13996. Tried the force of pieces of *iron*, *nickel* and *cobalt* with the present fine silver torsion wire, the pole pieces of the magnet being open to the full extent of 4·7 inches as before (13986½).

13997. *Iron.* A thinner wire than the former (13987), being $\frac{1}{90}$ or 0·011 of an inch in diameter, was annealed and employed, a piece about 0·1 of an inch in length being put into the copper cube as before (13987). It required about 7 revolutions of the torsion index to carry it from one upsetting point to the other (14072). The cube, iron and its wire and loop weighed 46 grains.

13998. *Nickel.* A cube of copper 0·25 of an inch in the side was suspended as in the former case by the fine copper wire. A piece of pure Nickel (13991), sawn off from the former piece by a copper plate and emery, was attached to the cube by soft cement; the nickel was about 0·1 or 0·09 of an inch in length and about 0·036 square. It pointed with such force as to require about 8 revolutions of the torsion index to carry it from one upsetting point to the other.

13999. *Cobalt.* A like copper cube had a piece of pure cobalt (13992) attached to it:–it was about 0·12 of an inch long and 0·14 of an inch square. It was far too strong for the wire; 5 revolutions of the torsion index did not displace the piece much. A smaller piece, about 0·08 of an inch long and 0·027 square, required nearly 6 revolutions from one upsetting point to the other. This piece will do.

14000. It will be best to use the platinum torsion wire (   ) for these metals:–the pieces here referred to will do, and if the torsion indication is not enough, it can be increased by bringing the pole pieces nearer together.

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14001. One of the varnished crystals of red ferro pruss. pot. (13995), being hung up in water, dissolved away fast–a stream of solution descended from it and in a little while only the varnish was left, forming a beautiful cast of the crystal; it broke on lifting it out, not being able to contain the weight of water within it. The varnish seemed as porous as a sieve. The red ferro pruss. is

soluble in the spirit of the varnish, as was seen at the time of varnishing—perhaps that is the reason they are now so porous. Revarnished the remaining crystals and hung them up in the warm air cupboard to dry.

14002. Ground off the ends and side edges of two crystals of red ferro prussiate—attached wires to them—dipped them into white hard varnish and hung them up to dry.

14003. Crystals of red ferro pruss. potash are insoluble and unaffected in camphine and also in olive oil.

14004. Wished to obtain a medium, and a crystal which, being in it, should be attracted bodily in one position and repelled in another. Worked first with the Tourmaline (13821, 2, 3). So constructed a torsion balance suspended by four fibres of Cocoon silk and having a lever of which the arms were about 2 inches long. By using the copperfoil suspender (13808) and copper foil edges at the ends of the lever arms, it was easy to have the crystal so that its axis should be either in one horizontal direction or the other.

14005. The *tourmaline in air*. Whether the Crystal axis was coincident to the magnetic axis or transverse to it, the tourmaline *was attracted*; but I think the attraction was distinctly the strongest when the Crystal axis was transverse—the transverse position is that which is taken up by magnecrystallic action when the crystal is free to move in any direction.

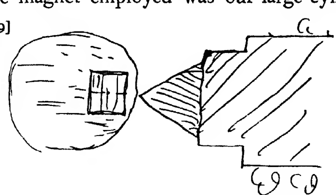
14006. The *tourmaline* in a saturated solution of *proto sulphate of iron*. Whether the crystal axis was coincident with or transverse to the magnetic axis, the tourmaline was attracted, but not so strongly in the first case as in the second. This solution of iron therefore does not supply the case I want, being less magnetic than the tourmaline in any position.

14007. The *tourmaline in Water*. As expected—the tourmaline more attracted than in the ferrugineous solution, or as I think in air. It was most attracted when its axis was equatorial or transverse.

14008. As a whole, the tourmaline was most attracted in the water and least in the iron solution. But when in the same medium, always attracted most when its axis was equatorial.

14009*. The magnet employed was our large cylinder Electro

* [14009]



magnet, excited when needed by 10 pr. of Grove's plates. The end of the core was terminated by an iron cone, so as to give a field of magnetic power rapidly diminishing in strength.

14010. *Red ferro pruss. potash.* I have ground off the ends and acute linear angles of a crystal of this salt, and suspended it by a copper wire as before (13979); then worked with it as just now with the tourmaline (14005). When in *air*, this crystal was *attracted*, whether its axis (which points equatorially) was coincident with or transverse to the magnetic axis. So it is magnetic, as Plücker says; and it is no use trying it in Camphine since there it must appear more magnetic than in air.

14011. Put the crystal into a saturated solution of proto sulphate of iron; it immediately became invested with a thin transparent film of prussian blue, not visible by its own colour but by its effect, for no solution of the red ferro prussiate descended or ascended into the iron solution, nor was any change produced in the latter. But a solution of the red ferro prussiate formed around the crystal slowly, between it and the inclosing film of prussian blue. It had the proper yellow colour and contained no prussian blue; the whole of the latter substance which was formed was in the film itself. This film had much osmotic action and the quantity of water which could pass through it to dissolve the crystal is probably very considerable.

14012. On making quick observations with the crystal (thus self protected to a considerable degree), I found that whether the Crystal axis were coincident with or transverse to the magnetic axis, still it was repelled. So here I shall, by the use of water, be able to obtain a medium corresponding to the mean power of the crystal—and then it ought to be attracted when Crystal axis transverse and repelled when coincident with the Magnetic axis.

14013. One vol. Sat. solution of sul. Iron and two vols. of water mingled and employed as the medium. The crystal wiped dry and then introduced into this solution. The film formed as before, but the surrounding solution of the salt formed more rapidly than in the former case.

14014. At first the Crystal axis coincided with the magnetic axis and there was attraction. When the Crystal axis was transverse, I could not be sure of the movements, for the solution of the

crystal had increased and was dripping down through the solution. The attraction ought to be stronger in this position and therefore a weaker solution will be required.

14015. Have varnished some crystals (14002) in hopes that they, by the aid of the prussian blue, will do (14026).

14016. A piece of granular bismuth on this torsion balance (14004) in melted phosphorus, under the power of this magnet (14009). I could not decide whether the metal was paramagnetic or diamagnetic in relation to the phosphorus. They must be near together.

14017. As media, Water and bisulphide of carbon are nearly alike in diamagnetic power. Olive oil is less diamagnetic than either.

14018. Experimented in relation to the equality of the differential Magnecrystallic force, whatever the medium around the crystal, and first worked with the *Carb. iron* (13976, 94), having the Logeman Magnet in use as before and the pole pieces wide apart (13977). Observed generally as in the former cases (   ).

14019. *Crystal of Carb. iron in air.* The torsion to the right was 266—that to the left was 277. The whole torsion between the upsetting points was 543. The observations were carefully made.

14020. *Carb. iron crystal in Water.* The right torsion was 265:—the left torsion 277. The whole torsion between the upsetting points 542.

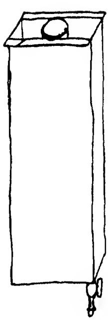
14021. *Carb. iron crystal* in *sat. solution proto sulphate of iron.* The right torsion was 265:—the left torsion 277:—the whole torsion 542.

14022. In all these cases therefore, the torsion was alike, being 543, 542, 542—notwithstanding the difference in the magnetic character of the surrounding media. It was not considered necessary to ascertain the angle between the upsetting points, as the correction for its amount would have made no difference in the equality of the corrected results.

14023. *Red ferro pruss. potassa.* The varnished crystal (13995, 79) was subjected to the action of the Logeman, within a copper trough about an inch in diameter, up to which the pole pieces were brought as on former occasions. When *Air* surrounded the crystal, the right torsion was 126—and the left torsion 188—the

whole torsion between the upsetting points being 314, including the upsetting angle. Then Camphine was poured into the copper trough, and now the right torsion was 126 and the left torsion 190–the whole torsion being 316, or the same as before. So what is true of Carbonate of iron and other magnecrystals is true of this crystal also. The differential force is not disturbed by variation of the medium.

14024. If the paramagnetic force of the medium round the crystal were very greatly increased, the differential magnecrystallic force of the crystal ought to be greater, for the effect would be as if a stronger magnet were employed. But if the medium spread every where else, as well as around the crystal, then this increase should disappear again, because the concentration of force near the crystal would now be removed and the power be uniform, or rather as at first.



14025. I have had two new copper troughs made, one hard soldered for hot oil, the other soft soldered for hot water. They are about 1·15 inches wide, 3·5 inches long and 7 inches deep–each has a small stop cock at the bottom and each is to have a cloathing of double flannel. I have also two protecting copper cylinders for them, closed at the bottoms.

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14026. The Varnished crystals of red ferro prussiate were not protected (14015), for both those with one and with two coats of varnish suffered when put into sol. of Sulphate of iron. The solution formed within the encasing squeezed out through various apertures, in strings ascending and descending, but not issuing laterally; these formed flexible bags or tubes encased in films of Prussian blue, and the solution continually forming about the crystals expanded into them. When a crystal is suspended in dilute solution of sul. Iron, 1 vol. Sat. sol. + 3 vols. water, the solution of red ferro prussiate which forms round the crystal is heavier than the solution of Iron, and at last the bag of it becomes so heavy that it falls off, or may be cut off by a wire, and sinks. After that endosmose goes on through the film of prussian blue; water only enters and dilutes the solution, and it soon becomes so light as to float in the solution in which it sank before. The effect

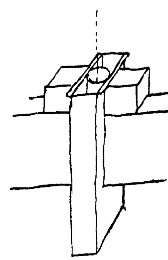
is very curious; and hence the ascending and the descending streams or long bags of solution spoken of above.

14027. The former glasses (14011–3) had been left to see what the action would be:–it had go[ne] on to an end and now only clots of prussian blue in a liquid were left.

14028. Renewed the expts. with crystals and Solution of sul. Iron (14011–5), using a varnished crystal and a medium consisting of 1 vol. Sat. sol. sul. iron, 3 vols. of water and a few drops of dilute Sulc. acid. As said (14026), the crystal was not protected by the varnish. It seemed to be nearly indifferent in the solution when with Crystal axis coincident with magnetic axis–but the air of the place affected the torsion balance and I must have that within a jar. Use also the great horse shoe magnet.

14029. Dipped some crystals in melted wax. Some in suddenly–others kept in until as warm as the melted wax. One of these in water was some time before it shewed any appearance of solution formation. I think this expedient will do. The crystals were of course the red ferro pruss. potassa (14051).

14030. Exptd. at *low temperatures* with the present fine silver torsion wire (13736) and the square pole faces against the trough, used as in the former cases. The trough was 3 inches deep, 4 inches long and 1·1 inches broad inside, and was cloathed in double flannel. A tight copper cylinder to contain the medium for the object was there, which was 2·7 deep and 1·05 inches in diameter. This trough and magnet pieces, being once adjusted, was not again disturbed whilst the experiments on the Tourmaline and bismuth crystal continued, that the power might be constant. When required for low temperatures, an excellent freezing mixture was put into the trough–the copper cylinder, containing cooled saturated brine, was put into its place and the object suspended in it. After a little practice, I found it easy to make a good observation at low temperatures. With each object a fresh cold mixture was used–but the same cylinder brine. Began first with the cylinder brine at common temperatures.



14031. *Bismuth crystal* (13592, 898). Temp. 69° F.

$$\left.\begin{array}{l}\text{Right torsion } 146\\ \text{left torsion } \quad 103\end{array}\right\} = 249 - 105 \text{ for upsetting angle } (13900) = \quad \Big|\ \begin{array}{l}\text{torsion}\\ 144 \text{ at } 69^\circ \text{ F.}\end{array}$$

14032. *Tourmaline crystal* (13691, 828). Temperature 69° F.

	Right torsion 330	1003 − 90 =	torsion fce. 913 at 69°
	Left ,, 673	1001 − 90 =	911 ,,
	Right ,, 328	999 − 90 =	909 ,,
temp. 69°·4 F.	left ,, 671		

14033. Now employed the *cold mixture and brine* (14030).

Tourmaline	right torsion 409 temp. 4°	1161 average temp. 7° F.	
		90	
	left ,, 752 ,, 10°	1071 torsion at 7° F.	

Mixed up salt, ice, etc. and observed again.

left torsion 757 temp. 10°	1152 average temp. 11° F.
	90
right ,, 395 temp. 12°	1062 torsion at temp. 11° F.

A fresh freezing mixture used and the *bismuth crystal* (14031) temp. 4° F.

14034.

Bismuth	right torsion 145		
	left ,, 124 temp. 5°	269 temp. 5° F. − 105 =	164 temp. 5°
	right ,, 145	269 ,, − 105 =	164 ,,
	left ,, 125 temp. 5°	270 ,, − 105 =	165 ,,

14035. The angle between the upsetting points is for the bismuth 105° (13900) and for the tourmaline 90° (13824). So the results are

14036.

	Bismuth crystal (14031, 4)
temp.	5° F. torsion = 164
	,, ,, ,, 164
torsion	,, ,, ,, 165
156 −41°	69° ,, ,, 144
143 −64°·8	
1385·5−79°	
(13898–902)	

Tourmaline (14032, 3)

temp.	7° F. torsion	= 1071 ÷ 3 =	357	−371·4	−390
	11° „	= 1062	354	−368	−386·3
	69° „	= 913	304·3	−316·2	−332
torsion 1022−39° 948−69° (13887)	„ „	= 911	303·6	−315·4	−331·2
	„ „	= 909	303	−315	−331

14037. Comparing the former bismuth results (13898–902) with those of to-day, the *latter are the highest*, for temp. 69° gives 144 torsion, whereas before, the lower temperature of 64°·8 gave only 143 torsion:—this is probably due to the poles being a little closer together to-day than on the former occasion. Had better lay these numbers down as a curve by the side of the former (13875, 98).

14038. Comparing the former tourmaline results (13887) with those of to-day, the *former are the highest*, judging by the effects at 69° F., for then they were 948 torsion and to-day 913. The difference I believe to be due to a like cause acting in the contrary direction. Lay these results down as a curve by the side of the former.

14039. *Crystal of Carb. iron* (13976, 7, 8) at temperatures between 4° and 66° F. Arranged the pole pieces wide apart (13977), with wooden blocks between them and the trough so as to keep all undisturbable when once arranged. Then worked for common temperature effects first, the crystal being as before in the brine. Temp. 66° F.

$$\left.\begin{array}{ll}\text{Right torsion} & 183\\ \text{Left}\quad\text{„} & 497\end{array}\right\}\ 680-96=584 \text{ at temp. } 66^\circ \text{ F. or} \div 2 = 292.$$

Altered the Zero so as to place it nearly intermediate.

$$\left.\begin{array}{ll}\text{Right torsion} & 344\\ \text{Left}\quad\text{„} & 337\end{array}\right\}\ 680\ [\textit{sic}]-\text{temp. } 66^\circ \text{ F.}$$

14040. Now applied the cold brine and bath as before (14030). Temp. 4° F.

Temp. 4°	Right torsion 429		
		849 − 96 = 753 at temp. 4° F. or ÷ 2 =	376·5.
	Left ,, 420		
		847 ,, ,, 751 ,, ,, ,,	375·5
,, 5°	Right ,, 427		
		845 ,, ,, 749 at temp. 5° F. ,,	374·5
	Left ,, 418		

14041. Very Good results. I must ascertain the upsetting angle of this crystal. The effect of temperature very considerable.

14042. The upsetting angle of the Carb. of iron is 96° (14067). The series further on (14068, etc.) will not join on badly to these results, for though the power is there a little less (622 in air at 62° and here 680 at 66°) because of a little difference in the width of the blocks, still the difference is small. Being here 849 at 4° (uncorrected for angle), it is there 340 for 293°. When corrected, the latter is not one third of the former, the numbers being 753 and 244 for torsion force.

14043. Proceeded to *heat experiments* with this torsion wire (14030) and an oil bath. Employed the bath described (14025), up to the sides of which the square faces of the pole pieces were brought (14030). A flannel covering without a jacket is on the trough, so that the bottom part can be pushed up, the trough bottom be protected by a sheet of copper with a square hole in it and then a lamp be applied to heat the trough and its contents. The copper cylinder has no hole in it, so that it is the medium cell and the trough is a bath (oil) for it. Olive oil was put into the medium cylinder and also into the bath trough and the *Compressed bismuth* (13975) was first attached to the torsion balance and observed at common temperatures and afterwards at higher temperatures. The upsetting points, though manifest enough, were not very definite and whilst adding more torsion, the crystal hung to its position, not turning over at once. This is probably due to the difference that must exist in a piece of matter so pressed. It cannot be like a crystal, i.e. every part cannot be equal in condition, and the part that is laterally outside cannot be under the same conditions of pressure as a part in the middle. The observations are somewhat confused by this indefiniteness in the upsetting points, but I did the best that I could. The arc between them may be taken experimentally as about 109°.

COMPRESSED BISMUTH

14044.

At		temp. was	F. 70°—right 146, left 120	= 266 − 109 =	157 at 70°
	3^h 8′ placed the small spirit lamp under the trough—then at temp. of 70°				
at	3.12′	temp. was	126°—for the oil heats fast enough—lamp away		
			—then right 142	= 249 − 109 =	140 at 121° average
,,	3.24	,, ,,	116° left 107	. . . . . .	. . . lamp on again—away
at	3.30	temp.	160° right 130	. . . . . .	temp. 162°—*currents*
				= 228 − 109 =	119 at 157° average
			154° left 98	. . . . . .	. . . lamp again at 3^h 38′
away at	3.46	,,	200° right 126	= 223 − 109 =	114 at 194° average
			188° left 97	. . . . . .	. . . at 3·54 lamp on—away *currents—currents*
at	4.0	,,	218° right 120, 204° left 95	= 215 − 109 =	106 at 211° average

14045. Considered these results as sufficient for the piece of compressed bismuth. The results stand as in the margin, and shew that here as before power is lost as the temperature rises. Must lay them down as a curve.

At 70°	torsion is	157
121°	,, ,,	140
157°	,, ,,	119
194°	,, ,,	116
211°	,, ,,	106

14046. Now removed the piece of compressed bismuth, and placed the *crystal of bismuth* (13592, 4031) on the balance, and went on still to raise the temperature—but the currents that occur are a serious hindrance to correct observation.

at	4^h 27^m temp.	202° —right 120			
			= 225 − 105 =	120 at temp. 209°	
	4.29 ,,	216° —left 105			
			= 218 − ,, =	113 ,, ,, 221°	
	4.30 ,,	*226°*[1]—right 113			
			= 214 − ,, =	109 ,, ,, 231°	
	4.32 ,,	236° —left 101			
			= 211 − ,, =	106 ,, ,, 240°	
	4.33 ,,	*245?* —right 110			
			= 204 − ,, =	99 ,, ,, 250°	
	4.34 ,,	254 —left 94			
			= 201 − ,, =	96 ,, ,, 259°	currents
	4.35 ,,	*263?* —right 107			
			= 202 − ,, =	97 ,, ,, 268	
	4.36 ,,	272 —left 95			
			= 198 − ,, =	93 ,, ,, 276°	torsion
	,,	*280* —right 103			

[1] Numbers in italics are in pencil in the MS.

14047.

[at	temp.	*280*	—right	103]		
					= 198 − 105 =	93 at temp. 284°
	,,	288°	—left	95		
					= 197 − ,, =	92 ,, ,, 285

4.40 the lamp flame was made small so as to allow slow cooling and now

	,,	*282*	—right	102		
					= 187 − ,, =	82 ,, ,, 279°
4.44	,,	275°	—left	85		
					= 187 − ,, =	82 ,, ,, 272°
	,,	*268*	—right	102		
					= 185 − ,, =	80 ,, ,, 265°
4.47	,,	261°	—left	83		
					= 186 − ,, =	81 ,, ,, 258°
	,,	*254*	—right	103		
					= 194 − ,, =	89 ,, ,, 251°
4.50	,,	247°	—left	91		
					= 198 − ,, =	93 ,, ,, 245°
	,,	*242*	—right	107		
					= 202 − ,, =	97 ,, ,, 240°
4.52	,,	237°	—left	95		
					= 202 − ,, =	97 ,, ,, 235°
	,,	*232*	—right	107		
					= 205 − ,, =	100 ,, ,, 230°
4.55	,,	228°	—left	98		
					= 210 − ,, =	105 ,, ,, 225°
	,,	*222*	—right	112		
					= 222 − ,, =	117 ,, ,, 219°
4.59	,,	216°	—left	110		
					= 222 − ,, =	117 ,, ,, 215°
	,,	*213*	—right	112		
					= 210 − ,, =	105 ,, ,, 212°
5.1	,,	211°	—left	98		
					= 212 − ,, =	107 ,, ,, 209°
5.3	,,	*206*	—right	114		
					= 218 − ,, =	108 [*sic*] ,, 204°
5.4	,,	201°	—left	104		
					= 221 − ,, =	116 ,, ,, 199°
5.5	,,	*198*	—right	117		
					= 224 − ,, =	119 ,, ,, 197°
5.7	,,	195°	—left	107		
					= 224 − ,, =	119 ,, ,, 193°
5.9	,,	*191*	—right	117		
					= 223 − ,, =	118 ,, ,, 190°
5.10	,,	188°	—left	106		
					= 226 − ,, =	121 ,, ,, 186° lamp quite away
	,,	*184*	—right	120		
					= 225 − ,, =	120 ,, ,, 183°
5.13	,,	181°	—left	105		
					= 224 − ,, =	119 ,, ,, 180°
	,,	*178*	—right	119		
					= 224 − ,, =	119 ,, ,, 177°
5.16	,,	175°	—left	105		

[at 5.16	temp.	175° –left 105]		
			= 233 – 105 =	128 at temp. 173°
5.20	,,	171 –right 128		
			= 241 – ,, =	136 ,, ,, 165°
5.22	,,	158° –left 113		
			= 239 – ,, =	134 ,, ,, 156°
	,,	154 –right 126		
			= 238 – ,, =	133 ,, ,, 152°
5.26	,,	150° –left 112		
			= 243 – ,, =	138 ,, ,, 149°
5.32	,,	147 –right 131		
			= 242 – ,, =	137 ,, ,, 141°
5.34	,,	134° –left 111		
			= 247 – ,, =	142 ,, ,, 133°
5.35	,,	132 –right 136		
			= 250 – ,, =	145 ,, ,, 131°
5.37	,,	130° –left 114		
5.45	,,	120 –right 135	= 256 – ,, =	151 ,, ,, 119°
5.48	,,	117° –left 121		
6.2	,,	105° –right 137	= 265 – ,, =	160 ,, ,, 104°
6.4	,,	103° –left 128		
6.27	,,	92° –right 142	= 280 – ,, =	175 ,, ,, 92°
6.30	,,	92° –left 138		

14048. As the angle between the upsetting points for this bismuth crystal is 105° (13900), the above have been corrected by that quantity.

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14049. The waxed crystal (14029) of red ferro prussiate which had been left in water—all the salt was now dissolved out.

14050. One of the crystals waxed a few days ago (14029) was put into a solution of proto sulphate of iron—prussian blue formed at once at the edges of the crystal and not at the faces, so that the form of the crystal was beautifully marked out. By degrees, fluid, i.e. water, entered here, solution was formed, rising tubes of P. Blue were produced from all parts of the edges; and these rose to the surface of the iron solution and served to carry up the new solution of the salt continually forming. All was very beautiful and interesting. There was perfect preservation of the crystal on the planes (14062).

14051. Fresh crystals of Red ferro pruss. pot. Dipped some into melted wax until they were as warm as the wax—took them out

and let them cool; then with a camel hair brush and melted wax, touched the linear edges of the crystals. This touching is a clumsy process—dipping the edges would be better.

14052. The tourmaline (13691) compared in different media (13693–700). The magnet and pole pieces and trough were just as left at the end of the experiment (14047), the copper cylinder being employed to contain the medium surrounding the tourmaline. The torsion is still measured by the fine silver wire (13736). The temperature was 65° F.

Tourmaline in Air

right torsion 536 } 1079
left torsion 543 }

Tourmaline in Alcohol

right torsion 534 } 1081
left torsion 547 }

Tourmaline in Water

right torsion 536 } 1082
left torsion 546 }

Tourmaline in Sat. solution proto sul. iron

Right torsion 534 } 1081
left torsion 547 }

Tourmaline in Olive oil

Right torsion 535 } 1085
Left torsion 550 }

14053. The motion in cold oil is very slow, and so, much care is required in ascertaining the upsetting points. If time and care be taken, the viscidity of the medium and the time enables one to go to the extreme force required before the upset takes place. In air it is the reverse, for it is hardly possible to prevent some trace of vibration or swing in the crystal, and so it is often swung up to and beyond the upsetting point, and so passes it too soon.

The result is as follows:

14054. Tourmaline crystal in Air—1079 torsion force between upsetting pts.
Alcohol—1081
Water—1082
Sol. Sul. Iron—1081
Oil—1085

so that the force required may be considered as the same for all

the media. The angle between the upsetting points has not been abstracted, inasmuch as that would make no difference in the result ( ). This angle is 90° (13824).

14055. Tourmaline (13691) at different temperatures in hot oil, the arrangement the same as that on the 30th Ult. (14043), and gives torsion 994 at temp. 65° F.

		F.					
at	$1^h 40^m$ temp.	65° spirit lamp put under the oil trough					
	1.50 ,,	120°—right	498		torsion ÷ 3 = ⟶		
				= 992 − 90 =	902 at temp. 128° F.		300·6
	1.53 ,,	136°—left	494	. . . .	. . . .	—temp. 148° at $1^h 56^m$	
				= 946 ,, =	856 ,, ,,	153°	285·3
	2.2 ,,	170°—right	452				
				= 902 ,, =	812 ,, ,,	173°	270·6
	2.5 ,,	177°—left	450	. . . .	. . . .	—temp. 181° at $2^h 6^m$	
				= 878 ,, =	788 ,, ,,	186°	262·6
	2.11 ,,	195°—right	428				
				= 863 ,, =	773 ,, ,,	197°	257·6
	2.13 ,,	199°—left	435	. . . .	. . . .	—temp. 203 at $2^h 14^m$	
				= 840 ,, =	750 ,, ,,	209°	250
	2.21 ,,	220°—right	405				
				= 825 ,, =	735 ,, ,,	223°	245
	2.24 ,,	226°—left	420				
	2.27 ,,	228°—					

14056. *Tourmaline in hot oil.* Fine silver torsion wire.

at	2.35 temp.	268°—right	363		torsion ÷ 3 = ⟶		
				= 736 − 90 =	646 at temp. 271° F.		215·3
	2.37 ,,	274°—left	373				
	2.40 ,,	280°—		= 728 ,, =	638 ,, ,,	282°	212·6
	2.43 ,,	291°—right	355	. . . .	. . . .	spirit flame reduced	
				= 723 ,, =	633 ,, ,,	289°	211
	2.47 ,,	286°—left	368				
	2.50 ,,	285°—		= 721 ,, =	631 ,, ,,	285°	210·3
	2.51 ,,	284°—right	353				
				= 717 ,, =	627 ,, ,,	284°	209
	2.53 ,,	284°—left	364	. . . .	. . . .	oil added at the top	
				= 724 ,, =	634 ,, ,,	280°	211·3
	2.57 ,,	276°—right	360				
				= 730 ,, =	640 ,, ,,	275°	213·3
	2.59 ,,	274°—left	370				
				= 733 ,, =	643 ,, ,,	274°	214·3
	3.1 ,,	274°—right	363				
				= 727 ,, =	637 ,, ,,	274°	212·3
	3.3 ,,	274°—left	364				
				= 728 ,, =	638 ,, ,,	274°	212·6
	3.5 ,,	274°—right	364				
				= 728 ,, =	638 ,, ,,	272°	212·6
	3.9 ,,	270°—left	364				
				= 730 ,, =	640 ,, ,,	269°	213·3
	3.11 ,,	267°—right	366	. . . .	. . . .	oil added above	

[at 3.11	temp.	267°–right	366]			torsion ÷ 3 = ⟶		
				= 741 – 90 =	651 at temp.	264° F.	217	
3.13	,,	260°–left	375					
				= 742 ,, =	652 ,, ,,	256°	217·3	
3.15	,,	252°–right	367					
				= 742 ,, =	652 ,, ,,	251°	217·3	
3.18	,,	250°–left	375					
				= 745 ,, =	655 ,, ,,	249°	218·3	
				. . . . .	. . . .		oil added above	
3.20	,,	248°–right	370					
				= 754 ,, =	664 ,, ,,	246°	221·3	
3.22	,,	244°–left	384					
				= 759 ,, =	669 ,, ,,	242°	223	
3.24	,,	240°–right	375					
				= 759 ,, =	669 ,, ,,	240°	223	
				. . . . .	. . . . vessel appears to leak out oil			
3.26	,,	239°–left	384					
				= 771 ,, =	681 ,, ,,	239°	227	
3.27	,,	238°–right	387					
				= 769 ,, =	679 ,, ,,	238°	226·3	
				. . . . .	. . . .		oil added above	
3.29	,,	237°–left	382					
				= 772 ,, =	682 ,, ,,	234°	227·3	
3.34	,,	230°–right	390					
				= 780 ,, =	690 ,, ,,	230°	230	
3.36	,,	229°–left	390					
				= 788 ,, =	698 ,, ,,	229°	232·6	
3.38	,,	228°–right	398					
				= 793 ,, =	703 ,, ,,	228°	234·3	
				. . . . .	. . . .		oil added above	
3.39	,,	228°–left	395					
				= 789 ,, =	699 ,, ,,	225°	233	
3.44	,,	222°–right	394					
				= 791 ,, =	701 ,, ,,	222°	233·6	
3.46	,,	222°–left	397					
				= 797 ,, =	707 ,, ,,	222°	235·6	
				. . . . .	. . . . . . lamp flame increases			
3.48	,,	223°–right	400					
				= 798 ,, =	708 ,, ,,	223°	236	
				. . . . .	. . . .		lamp put out	
3.50	,,	224°–left	398					
				= 796 ,, =	706 ,, ,,	222°	235·3	
3.55	,,	219°–right	398					
				= 803 ,, =	713 ,, ,,	218°	237	
3.57	,,	216°–left	405					
				= 808 ,, =	718 ,, ,,	212°	239·3	
3.59	,,	207°–right	403					
				= 818 ,, =	728 ,, ,,	205°	242·6	
				. . . . .	. . . .		oil added above	
3.60	,,	203°–left	415					
				= 819 ,, =	729 ,, ,,	199°	243	
4.2	,,	194°–right	404					
				= 824 ,, =	734 ,, ,,	191°	244·6	
4.4	,,	188°–left	420					
				= 837 ,, =	747 ,, ,,	184°	249	
4.6	,,	180°–right	417					
				= 845 ,, =	755 ,, ,,	178°	252·6	
4.7	,,	176°–left	428					
				= 847 ,, =	757 ,, ,,	174°	252·3	
4.8	,,	172°–right	419					
				= 859 ,, =	769 ,, ,,	170°	256·3	
4.9	,,	168°–left	440	. . . . .	. . . .		oil added above	

[at 4.9	temp.	168°—left	440]		torsion ÷ 3 = ⟶		
				= 874 − 90 =	784 at temp.	165° F.	261·3
4.11	”	162°—right	434				
				= 879 ” =	789 ” ”	161°	263
4.12	”	159°—left	445				
				= 888 ” =	798 ” ”	157°	266
4.13	”	155°—right	443				
				= 901 ” =	811 ” ”	154°	270·3
4.14	”	152°—left	458				
				= 898 ” =	808 ” ”	151°	269·3
4.16	”	149°—right	440				
				= 908 ” =	818 ” ”	148°	272·6
4.17	”	147°—left	468				
				= 908 ” =	818 ” ”	146°	272·6
4.18	”	144°—right	440				
				= 905 ” =	815 ” ”	143°	271·6
4.19	”	142°—left	465				
				= 913 ” =	823 ” ”	141°	274·3
4.20	”	140°—right	448	. . . . .	. . . . .	. .	oil added above
				= 926 ” =	836 ” ”	138°	278·6
4.23	”	136°—left	478				
				= 938 ” =	848 ” ”	135°	282·6

14057. *Tourmaline in hot oil.*

				= 938 ” =	848 ” ”	135°	282·6
4.24	”	133°—right	460				
				= 935 ” =	845 ” ”	132°	281·6
4.25	”	131°—left	475				
				= 939 ” =	849 ” ”	130°	283
4.26	”	129°—right	464				
				= 945 ” =	855 ” ”	128°	285
4.28	”	126°—left	481				
				= 956 ” =	866 ” ”	125°	288·6
4.30	”	123°—right	475				
				= 959 ” =	869 ” ”	122°	289·6
4.32	”	120°—left	484				
				= 959 ” =	869 ” ”	119°	289·6
4.33	”	118°—right	475				
				= 972 ” =	882 ” ”	117°	294
4.36	”	116°—left	497	. . . . .	. . . . .	. .	oil added above
				= 978 ” =	888 ” ”	115°	296
4.37	”	113°—right	481				
				= 978 ” =	888 ” ”	113°	296
4.38	”	112°—left	497				
				= 982 ” =	892 ” ”	111°	297·3
4.40	”	110°—right	485				
				= 978 ” =	888 ” ”	109°	296
4.43	”	107°—left	493				
				= 985 ” =	895 ” ”	106°	298·3
4.48	”	104°—right	492				
				= 997 [*sic*] =	907 ” ”	101°	302·3
4.56	”	98°—left	509				
				= 1007 ” =	917 ” ”	97°	305·6
4.58	”	97°—right	498				
				= 1018 ” =	928 ” ”	96°	309·3
4.59	”	96°—left	520				
				= 1019 ” =	929 ” ”	96°	309·6
4.60	”	96°—right	499				

					torsion ÷ 3 = ——→		
at 5.18	temp.	89°–left	521	= 1039–90 =	949 at temp.	88° F.	316·3
5.20	„	88°–right	518	= 1041 „ =	951 „ „	88°	317
5.21	„	88°–left	523	= 1033 „ =	943 „ „	87°	314·3
5.23	„	87°–right	510				
5.40	„	83°–left	537	= 1063 „ =	973 „ „	83°	324·3
5.42	„	83°–right	526	= 1065 „ =	975 „ „	83°	325
5.43	„	83°–left	539	= 1056 „ =	966 „ „	82°	322
5.44	„	82°–right	517				
6.6	„	79°–left	549	= 1071 „ =	981 „ „	79°	327
6.10	„	79°–right	522	= 1058 „ =	968 „ „	79°	322·6
6.12	„	79°–left	536	= 1060 „ =	970 „ „	79°	323·3
6.13	„	79°–right	524				

14058. The running away of the oil appears to have been caused by the hot fluid having crept up by and between the thermometer stem and the trough, so as to reach the flannel jacket, which then has become oiled and dripping all down to the bottom and acted like a syphon. Hence the running over effect, which occurred chiefly when the temperature was high and when oil films run freely over surfaces, and ceased as the temperature fell and the oil level was allowed to lower. Must not make the oil level in the trough too high.

14059. The numbers run on very well. It is easily seen at the temperatures above 128° F. how much stronger the rising numbers for force are than the falling numbers, the tourmaline being behind the thermometer in the attainment of temperature (14046). The falling indications are most to be trusted because more time is occupied in cooling than in heating, and there are weaker disturbing currents.

14060. Must lay down the Tourmaline curve.

14061. The length of the torsion wire is such that when the tourmaline crystal is hanging to it, the bottom of the plate suspending hook is just 23·2 inches below the under surface of the wooden top supporting the torsion index.

14062. *Red ferro pruss. potassa.* The crystal (14050) was now all gone and only Prussian blue remaining—except the wax case, which was very thin and imperfect at the edges, though thick on the flat faces. This is the effect of what we call capillary attraction and offers some beautiful illustrations of that power. A little dampness or other such cause would soon make such a thinned film break through whilst fluid.

14063. The crystals touched at the edges (14051), being put into a solution of Sulphate of iron, immediately shewed some places where the surface of wax was broken and penetrated: but many of the edges were well preserved. Still, it will not do.

14064. The crystals dipped at the edges (14051) were like the last, only imperfectly protected.

14065. Some crystals were dipped into melted wax and kept there until they were as warm as the liquid, then taken out, the hanging drop taken off by a piece of paper, and the crystals allowed to cool for an hour. They were then dipped quickly into and out of wax which was cooled until part was set. This covered them with a second good coat over the first. After 2 or 3 hours, put these into sol. sulphate of iron. They stand by much the best, but there are a few points from which solution and then tubes of P.B. ascend—these would impede motion and such crystals will not do.

14066. Took some crystal[s] and ground them down, removing all the edges and angles and irregular parts of the surface, and reducing them to rounded mass. The object was to do away with the angles where the liquid film thins out (14062). Dipped these in hot melted wax until they were heated—cooled them—they seemed to be well coated at every part. Dipped them quickly into wax partly solid, and then cooled them for an hour or two. One of these hung up in the ferruginous solution gave no signs of action after some hours—left it to see the action of time. It will do (14088).

14067. *Crystal of Carb. of Iron* (13976, 4039). Proceeded to work with it for effects of temperature, using the oil bath (14043, 55), and its cylinder. The pole pieces were 4·86 inches apart, with blocks between them and the oil bath. The oil surface was kept lower, about $\frac{1}{2}$ an inch below the trough edge, and there was no

running over of the oil (14058). When all things in position, ascertained first the torsion force in *Air*, the temperature being 62° F.

To the right the force was 311 }
left „ „ 311 } 622 − 90 = 532 *at* 62° *being in air.*
÷ 1·8 = 296

The upsetting angle was ascertained to be 96°.

14068. Put oil into the cylinder round the crystal and began to observe in it.

		F.					
At	9h 23m—temp.	62°—right	312	623 − 90 =	533 at temp. 62° ÷ 1·8 = 296·3		
		left	311				
	9.40 „	the spirit lamp put beneath the oil vessel					
	9.52 „	173°					
	10.12 „	270°					
	10.14 „	276°—right	182	. . . . .	. ÷ 1·8 = ——→		lamp away awhile
				364 − 96 =	268 at temp. 266° F.	149	
	10.22 „	256°—left	182				
				364 „ =	268 „ „ 264°	149	
	10.28 „	272°—right	182				currents—strong
				366 „ =	270 „ „ 274°	150	
	10.29 „	277°—left	184				
				361 „ =	265 „ „ 283°	147·5	
	10.35 „	289°—right	177				
				351 „ =	255 „ „ 285°	142	
	10.36 „	282°—left	174	. . . . .	. . . .	.	lamp increased
				353 „ =	257 „ „ 281°	143	
	10.38 „	280°—right	179				
				352 „ =	256 „ „ 282°	142·3	
	10.40 „	285°—left	173				
				344 „ =	248 „ „ 287°	138	
	10.42 „	290°—right	171				
				341 „ =	245 „ „ 291°	136·3	
	10.44 „	293°—left	170				
				339 „ =	243 „ „ 293°	135	currents
	10.45 „	294°—right	169				
				341 „ =	245 „ „ 291°	136·3	
	10.48 „	288°—left	172				
				345 „ =	249 „ „ 286°	138·5	

14069. *Carb. iron crystal at different temperatures.*

			345 „ =	249 „ „ 286°	138·5
10.49 „	283°—right	173			
			351 „ =	255 „ „ 282°	141·8
10.51 „	280°—left	178			
			350 „ =	254 „ „ 279°	141·2
10.52 „	278°—right	172			
			347 „ =	251 „ „ 277°	139·5
10.54 „	276°—left	175			
			347 „ =	251 „ „ 276°	139·5
10.55 „	275°—right	172			

[At	10.55	temp.	275°—right	172]					divided by 1·8 gives	
					349 − 96 =	253	at temp.	274° F.	140·7	
	10.58	,,	273°—left	177						
					358 ,, =	262	,, ,,	273°	145·6	
	11.1	,,	272°—right	181						
					361 ,, =	265	,, ,,	272°	147·3	
					. . . . .	. . . . .	. . .			lamp reduced
	11.2	,,	271°—left	180						
	11.5	,,	267°							
					361 ,, =	265	,, ,,	268°	147·3	
	11.6	,,	264°—right	181						
					363 ,, =	267	,, ,,	263°	148·4	
	11.8	,,	262°—left	182						
					365 ,, =	269	,, ,,	261°	149·5	
	11.10	,,	260°—right	183						
					367 ,, =	271	,, ,,	259°	150·7	
	11.13	,,	257°—left	184						
					371 ,, =	275	,, ,,	256°	153	
	11.18	,,	254°—right	187						
					376 ,, =	280	,, ,,	252°	155·7	currents
	11.21	,,	250°—left	189						
					377 ,, =	281	,, ,,	249°	156·2	
					. . . . .	. . . . .	.			no oil running over
	11.22	,,	247°—right	188						
					379 ,, =	283	,, ,,	246°	157·3	
	11.25	,,	245°—left	191						
					384 ,, =	288	,, ,,	244°	160	
	11.30	,,	242°—right	193						
					386 ,, =	290	,, ,,	241°	161·3	
					. . . . .	. . . . .	. . .			put oil in above
	11.33	,,	239°—left	193						
					386 ,, =	290	,, ,,	236°	161·3	
	11.37	,,	232°—right	193						
					389 ,, =	293	,, ,,	232°	163	
					. . . . .	. . . . .	. . .			smaller flame
	11.39	,,	232°—left	196						
					390 ,, =	294	,, ,,	232°	163·5	
	11.43	,,	231°—right	194						
					396 ,, =	300	,, ,,	229°	166·9	
	11.46	,,	226°—left	202						
					407 ,, =	311	,, ,,	223°	173	
	11.51	,,	219°—right	205						
					408 ,, =	312	,, ,,	218°	173·5	
	11.53	,,	216°—left	203						
					403 ,, =	307	,, ,,	214°	170·8	
	11.57	,,	211°—right	200						
					411 ,, =	315	,, ,,	210°	175·1	
	12.0	,,	208°—left	211						
					413 ,, =	317	,, ,,	207°	176·2	
	12.2	,,	206°—right	202						
					410 ,, =	314	,, ,,	205°	175·5	
	12.4	,,	204°—left	208						
					413 ,, =	317	,, ,,	203°	176·2	
	12.6	,,	202°—right	205						
					414 ,, =	318	,, ,,	201°	177	currents
	12.8	,,	200°—left	209						
					413 ,, =	317	,, ,,	199°	176·2	
	12.12	,,	198°—right	204						
					417 ,, =	321	,, ,,	197°	178·4	
	12.16	,,	195°—left	213						

[At 12.16	temp.	195°–left	213]					divided by 1·8 gives
				427 − 96 =	331 at temp.		194° F.	184
12.22	”	192°–right	214					
				429 ” =	333 ”	”	191°	185·2
12.28	”	189°–left	215					
				. . . . .	. . . . .		. lamp away altogether	
				427 ” =	331 ”	”	188°	184
12.31	”	187°–right	212					
				429 ” =	333 ”	”	184°	185·2
12.34	”	181° left	217					
				427 ” =	331 ”	”	179°	184
12.36	”	177°–right	210					
				435 ” =	339 ”	”	175°	188·5
12.38	”	172°–left	225					
				445 ” =	349 ”	”	170°	194
12.40	”	167°–right	220					
				448 ” =	352 ”	”	164°	195·8
12.42	”	161°–left	228					
				454 ” =	358 ”	”	159°	199
12.44	”	157°–right	226					
				455 ” =	359 ”	”	155°	199·6
12.46	”	153°–left	229					
				458 ” =	362 ”	”	151°	201·2
12.48	”	149°–right	229					
				469 ” =	373 ”	”	147°	207·5
12.50	”	145°–left	240					
				480 ” =	384 ”	”	144°	214·5
12.52	”	142°–right	240					
				483 ” =	387 ”	”	140°	215·2
12.54	”	138°–left	243					
				485 ” =	389 ”	”	137°	216·3
12.56	”	135°–right	242					
				488 ” =	392 ”	”	134°	218

14070. *Carb. iron crystal at different temperatures.*

				488 ” =	392 ”	”	134°	218
12.58	”	132°–left	246					
				489 ” =	393 ”	”	131°	218·5
1.0	”	129°–right	243					
				501 ” =	405 ”	”	126°	225·4
1.4	”	123°–left	258					
				515 ” =	419 ”	”	122°	233
1.7	”	120°–right	257					
				517 ” =	421 ”	”	118°	234·1
1.10	”	116°–left	260					
				519 ” =	423 ”	”	114°	235·3
1.14	”	112°–right	259					
				522 [*sic*] =	426 ”	”	110°	237
1.18	”	108°–left	265					
				533 [*sic*] =	437 ”	”	106°	243
1.23	”	104°–right	271					
				546 ” =	450 ”	”	102°	250
1.28	”	100°–left	275					
				549 ” =	453 ”	”	99°	251
1.33	”	98°–right	274					

[At						divided by 1·8 gives		
	1.33	temp.	98°–right	274				
					559 − 96 =	463 at temp.	96°	257·5
	1.40	,,	93°–left	285				
					570 ,, =	474 ,, ,,	91°	263·7
	1.51	,,	89°–right	285				
					577 ,, =	481 ,, ,,	88°	267·5
	2.0	,,	86°–left	292				
					582 ,, =	486 ,, ,,	84°	270·1
	2.10	,,	82°–right	290				
	2.35	,,	76°–left	305				
					599 ,, =	503 ,, ,,	76°	280
	2.37	,,	76°–right	294				
	3.3	,,	73°–left	301				
					606 ,, =	510 ,, ,,	73°	283·7
	3.4	,,	73°–right	305	. . . .	. . . . . .		oil action–all well

14071. Took off the silver torsion wire (13736, 807) employed continually up to this time and put on a platinum torsion wire (13735, 14000), after having straightened out much of its curl by pulling it through fine sand paper in a straight direction–got it into its place, and adjusted its flat hook, length, etc. all very successfully.

14072. *Iron* (13997, 14000). A piece of the iron wire described and 0·1 (about) in length being in the copper cube–the object was then suspended from the new platinum torsion wire (14000), the pole faces separated to 3·8 inches apart and the torsion force between the upsetting points observed; it was 750 degrees. When the magnetic field was diminished to 2·2 inches the torsion force was 5^{r} 260 = 2060 degrees. When it was enlarged, the poles faces being separated 4·8 inches, then the torsion force was 500 degrees.

14073. I think an interval of about 3·5 inches will do for the Iron, Nickel and Cobalt.

14074. *Iron*–the upsetting points are nearly 90° apart and are very definite–is 108° ( ).

14075. *Fibrous iron* may be considered as a magnecrystallic body. Try a cube filed out of fibrous iron, or a cylinder turned.

14076. Query the effect of heat on iron? Is it not two fold, i.e. in relation to iron which can take any degree of permanent charge? For if we consider a perfectly hard magnet–all parts being alike hard and magnetized–a high heat destroys the charge;

if a moderate heat affects it and diminishes the charge, then a question arises, is the diminution permanent or is the full charge restored on lowering the temperature? Either answer would be important in the consideration of the nature of steel magnetic charge.

14077. If iron, perfectly soft, and able to take no charge were employed, then we require to know whether it loses power by increase of temperature and regains it by restoration to its first temperature—which I suppose it will do just as bismuth or carbonate of iron does.

14078. If iron be able to take and keep a degree of magnetic charge, or rather if a mixed mass of hard steel charged and soft iron wire were used, and the steel be unchangeable by heat, then the torsion force ought to increase with heat, because the iron would sink as a conductor. If the steel and the iron both lost and *regained* their respective powers by heat and cold, then the mixed mass should do so. If the steel lost permanently, the whole mass should lose permanently.

14079. Many interesting points would arise here for consideration—and also in like respects of Nickel and Cobalt—and of Iron considered as soft but yet able to take a degree of magnetic charge.

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14080. *Bismuth.* Comparing these together, they agree very well. The mean line of curve[1] appears to be a straight line or nearly so, for if the curve of the second set of observations is slightly convex it is hardly sensible in these results—and the lines are close together and nearly parallel. The crystal (13898–910–14036) had at 5° temp. a torsion force of 168 and at 207° a torsion force of only 90, the whole loss for the difference of 202° being 78, or nearly one half of the torsion force at 5°, and at the rate of 0·386 per temperature degree, whether high or low.

14081. The other Bismuth crystal observations (14046, 7) had at 92° a torsion force of 166 and at 280° a torsion force of 77 only, the whole loss for the difference of 188° being 89, i.e. rather more

[1] See the notes following pars. 13888 and 13902 regarding the curves referred to in pars. 14080–87.

than half the force it had at 92°, the loss being 0·473 per degree of temperature. If the mean line be struck as a curve upon the top and bottom parts, then a degree of temperature between 100 and 110° would probably cause a change of 0·56 of a degree of torsion force and a degree between 270° and 280° a change of 0·4 of a degree of force.

14082. The compressed bismuth (14045) had at 70° a torsion force of 159 and at 210° a torsion force of 105—the loss of force being 54 for the difference of temperature = 140°. This again is at the rate of 0·385 of a torsion degree for each degree of F.—being equable apparently throughout.

14083. If the three observations be taken for their respective losses between the *same* temperatures, then the results accord exceedingly well and shew that compressed bismuth is in the same condition as a crystal of bismuth. Thus:

		at 90°	at 207°		
the first observations,					
Crystal A (13898–910)	gives	135	90	the difference is	$\frac{45}{135} = \frac{1}{3}$
the second observations					
(14046) C	,,	167	112	,, ,,	$\frac{55}{167} = \frac{1}{3}$
the compressed bismuth					
(14045) D	,,	149	107	,, ,,	$\frac{42}{149} = \frac{1}{3\cdot5}$

being in the first case $\frac{1}{3}$ of the force at 90°—in the second a little less, and in the third close upon a third; thus shewing the near accordance of the results both with the bismuth crystd. and compressed.

14084. The *Tourmaline* gives a series decreasing with rising temperatures; the proportion of loss between 10° and 20° is above three fold what it is between 280° and 290°—the numbers being as 2·8 and 0·9 for each degree of F. It had at 7° a torsion power of 1108 and at 210° a torsion power of 697 only, so that 411 degrees of torsion force was lost during the rise of temperature through these 203 degrees of heat, being above a third of the whole force. In the other case (14054, 7), the torsion being 1011 at 65°, was reduced to 631 at 285°—the loss of 380 being above a third of the whole force at 65° F.

14085. Taking the two sets of observations between 65° and 210°, the results are

	at 65°	at 210°		
Obs. (13897)	958	697	diminution	$261 - \frac{261}{958} = 0{\cdot}272$ of the force at 65°
Obs. (14054)	1011	715	"	$296 - \frac{296}{1011} = 0{\cdot}292$ " " 65°

being a very good accordance.

14086. *The crystd. carbonate of iron.* This also gives a decreasing series. The loss between 10° and 20° F. is four times as much as that between 270° and 300°. The whole loss between 4° and 293° is equal to 654 − 246, or 408 torsion force, i.e. two thirds nearly of the power at 4°. As the powers at 65°, 210° are 516 and 307, so the loss there is 209 and $\frac{209}{516} = 0{\cdot}405$. So that it loses much faster at these temperatures than Tourmaline.

14087. The curve selected is merely approximative but I think that the numbers about 284 and 294 are near the truth.

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14088. Red ferro prussiate potash dipped in wax (14065, 6). The angular one (14065) was about half gone. The rounded one (14066) was vulnerable only at one spot, apparently an air hole—a tube (very fine) of solution and Prussian blue rose up from the spot on the next morning (the 7th) and by to-day that tube goes on delivering a little solution to the top. Every where else it is perfectly preserved. The process will do.

14089. *Iron.* Worked with the iron and cube before described (14072, 3, 4) with an interval between the pole faces of 4·86 inches. The upsetting angle is 108°. The torsion wire is the new platinum wire (14071). The common temperature is 65° F. The following result is in *Air*.

at	. . temp:	64°—right	224	456 − 108 =	348 at temp. 64° × 1·2 417·6
		left	232		

Then put oil into the cylinder, it being before in the bath (   ) and proceeded to work at different temperatures. The *iron and copper cube in oil.*

		F.			
at	11h 44m temp.	64°—right	224	462 − 108 =	354 at temp. 64 in the oil—425
		—left	238		
	11.50 . . . the spirit lamp put beneath the bath. The set of the iron				

is strong and the cube comes to its place vigorously vibrating, in air much and short. In oil also the position is well taken up.

at 11.55	temp.	97°–right	224				× 1·2
				461 – 108 =	353 at temp.	97°	424
11.56	„	–left	237		. . .	temp.	117°
12.25	„	257°–right	223				
				460 – „ =	352 „	„ 257°	423
		–left	237				
12.30	„	268°–right	226				
				463 „ =	355 „	„ 268°	426
		–left	237		. . .	temp.	272°
12.37	„	287°–right	226				
				462 „ =	354 „	„ 287°	425
		left	236		. . .	temp. 290°– lamp away.	

currents

14090. Thus it seems that there is no sensible difference between the force at 64° and 290°. Further on will be results at 32°; will add them here presently. See (14102). These give a close average of 348 (or 417·6) torsion force at 33°, which is 6 degrees *less* than the average at high temperatures. But the observation with cobalt[1] (14099) applies to the case.

14091. The cube was revolved 180°, so that any magnetism that the iron might hold should shew its effect, if any, in counteracting or favouring the induction result on the iron. The whole amount of torsion taken immediately between the upsetting points was 1^r 105° or 465°, which may be considered the same as above. So no permanent charge of the iron ungoverned by the induction of the magnet was present. The iron with its cube removed for the present (14102).

14092. The Cobalt and cube (13999) put on the balance under the same circumstances as the iron. The upsetting angle is 118°.

at 1^h 33^m	temp.	128°					
1.47	„	112°–right	140				× 2·2
				278 – 118 =	160 at temp.	111°	352
1.49	„	110°–left	138				
				277 „ =	159 „	„ 110°	350
		–right	139				
				278 „ =	160 „	„ 109°	352
	„	109°–left	139		. . .	temp.	109°
1.53	„	Spirit lamp under the bath					

1 ? nickel.

Cobalt at different temperatures.

							× 2·2
at	2.15	temp.	240°—right	148			
					292 − 118 =	174 at temp. 245°	383 currents
			—left	144			
	2.18	”	250°—right	147			
					290 ” =	172 ” ” 255°	378
			—left	143			
	2.21	”	260°—				
	2.24	”	270°—				
	2.30	”	284°—right	153			
					299 ” =	181 ” ” 284°	398
		”	285°—left	146			
					299 ” =	181 ” ” 286°	398
	2.32	”	288°—right	153			
					299 ” =	181 ” ” 289°	398
		”	290°—left	146			
					300 ” =	182 ” ” 289°	400
		”	288°—right	154			
					299 ” =	181 ” ” 287°	398
		”	285°—left	145			
					298 ” =	180 ” ” 286°	396
	2.36	”	286°—right	153			
					298 ” =	180 ” ” 289°	396
		”	292°—left	145			

14093. The results seem to be highest for the high temperatures. Will leave room for the cold results (14101) and then compare the whole.

14094. The cold results	(14101) give	155° ” ” 33°	341
and also	(14105) ”	155 ” ” 66°	341

14095. So the results above are anomalous, i.e. the force is greater at higher temperatures than at lower, being 181 at 287° and only 160 at 110°. That it should be still less at 66° and 33° may be otherwise accounted for.

14096. The effect may be due to the Cobalt having a trace of charge in the opposed direction which the higher temperatures partly dissipated. Must repeat the experiment: heating the cobalt first to redness (14112).

14097. The *Nickel* and *its copper cube* (13998) on the balance. The upsetting angle is about 112°.

at							×1·2	
2.43—temp.	272°							
2.47 ,,	277°—right	207						
			392 − 112 =	280 at temp.	278°	336	316·8
,,	280°—left	185						
			393 ,, =	281 ,, ,,	282°	337	317·6
2.50 ,,	284°—right	208						
			395 ,, =	283 ,, ,,	286°	339·6	320·3
,,	289°—left	187						
			394 ,, =	282 ,, ,,	290°	338·4	319·0
,,	292°—right	207						
			393 ,, =	281 ,, ,,	293°	337	317·6
2.54 ,,	294°—left	186	. . . . .	. . . . .	took away the lamp		
3.4 ,,	246°—right	209						
			399 ,, =	287 ,, ,,	243°	344·4	325·2
,,	240°—left	190						
			399 ,, =	287 ,, ,,	238°	344·4	325·2
3.6 ,,	235°—right	209						
			399 ,, =	287 ,, ,,	236°	344·4	325·2
,,	231°—left	190						
3.15 ,,	201°—right	209						
			399 ,, =	287 ,, ,,	199°	344·4	325·2
,,	196°—left	190						
3.30 ,,	160°—right	210						
			402 ,, =	290 ,, ,,	159°	348	328·4
,,	158°—left	192						
3.43 ,,	136°—right	211						
			402 ,, =	290 ,, ,,	135°	348	328·4
,,	134°—left	191						
4.6 ,,	109°—right	211						
			404 ,, =	292 ,, ,,	109°	350·4	330·7
,,	left	193						
4.30 ,,	95°—right	211						
			404 ,, =	292 ,, ,,	95°	350·4	330·7
,,	left	193						

14098. There then appears to be an increase at low temperatures. Took out the oil from the bath and cylinder without disturbing their positions or that of the magnet poles. Put ice into the bath with water and ice cold water into the cylinder round the *Nickel*—left all for half an hour and then observed again.

at						
5.30—temp.	33°—right	205				
			391 − 112 =	279 at temp. 33°		average 278 torsion × 1·2 = 333·6–314·7
	left	186				
			391 ,, =	279 ,, ,,		
	right	205				
			389 ,, =	277 ,, ,,		
	left	184				
			389 ,, =	277 ,, ,,		
	right	205				
			390 ,, =	278 ,, ,,		
	left	185	. . . .	. . . .	. Nickel removed	

14099. So it would appear that Nickel, in sinking in temp. from about 290° to 95°, gained in magnetic power, the forces being 282

and 292, and I think the result is correct. In the experiments at 33° F. it is true that diminution of force seems to appear—but the nickel had to be taken out and replaced—and still more in drawing off the oil and replacing it by ice and water, the trough had to be tilted a little between the poles—and I think this must have opened the pole pieces slightly and so caused the diminution of force.

14100. Assuming this to be true, then the diminution of Nickel force between 95° and 290° is 10 degrees of torsion force or $\frac{10}{292}$ of the force at the temperature of 95.

14101. *Cobalt.* The *cobalt and cube* replaced the Nickel and cube just dismissed, and the observations made with it were as follows:

at	5h 40m—temp.	33°—right	132			× 2·2
				273 − 118 =	155 at temp. 33	341
	,,	,, —left	141			
				273 ,, =	155 ,, ,,	341 Average
	5·47 ,,	,, —right	132			155–341
				273 ,, =	155 ,, ,,	341
	,,	33°—left	141			

14102. The *iron and cube* (14089) was put in place in this cold bath—the Cobalt being removed—the results were:
See (14089, 90) backwards.

at	5h 56m—temp.	33°—right	205			
				456 − 108 =	348 at temp. 33°	
		left	251			
				457 ,, =	349 ,, ,,	
		right	206			
				458 ,, =	350 ,, ,,	average 348
		left	252			
				454 ,, =	346 ,, ,,	
		right	202			
				455 ,, =	347 ,, ,,	
	6.5 temp.	33°—left	253			

14103. Now removed the ice by warm water and the stop cock, and after warming the bath put water at 70° nearly into it and the cylinder, and placed the *Nickel* and cube (14097) on the balance—after a time observed.

at	6h 27m—temp.	66°—right	201			
				387 − 112 =	275 at temp. 66°	
		left	186			
				389 ,, =	277 ,, ,,	
		right	203			
				389 ,, =	277 ,, ,,	average 276·6
		left	186			
				389 ,, =	277 ,, ,,	
		right	203			
				389 ,, =	277 ,, ,,	
		left	186			

14104. The result is a little less than that at temp. 33° which is 278—and in that respect accords with the general result, assuming that there has been no displacement of the poles.

14105. Placed the *Cobalt* in this bath and temperature.

$6^h\ 40^m$—temp.	66°—right	131			× 2·2
			272 − 118 =	154 at temp. 66°	339
	left	141			
			273 ,, =	155 ,, ,,	341
	right	132			
			273 ,, =	155 ,, ,,	341
	left	141			

This is the same result with that obtained at 33° F., which was 155—and both shew that the poles have not changed since (14099).

14106. These three metals, *Iron*, *Nickel* and *Cobalt*, can all take magnetic charge in the field, which they retain with a certain power. A piece of the iron wire, heated red hot, was when examined by a minute magnetic needle, without charge—held in the magnetic field a moment and brought away, it retained a charge, being magnetic. When held in the contrary position in the field, the magnetism was reversed, but a part of the reversed charge was brought away. The same was the case with the *Cobalt* and *Nickel*. This with their shape will aid in explaining why the upsetting angle[s] are so large, being for Iron 108—Nickel 112—and Cobalt 118. The iron ought to be the least as it is thinnest—the nickel and cobalt are both square prisms, length not above twice or thrice the width, and so the diagonal or longest line will form different angles with the axis of the pieces.

14107. The no change with iron (14090) shews that the force of the magnet is not affected by the heat of the oil bath so as to cause any of the effects with the other bodies.

14108. The thick torsion wire is good in that it makes the object more independant of the currents in the fluid. Though the degrees are less numerous, still the expression of them at the upsetting points is more accurately obtained.

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14109. The red ferro pruss. crystal (14066, 88)—preserved to the same extent as before except that it has now begun to give way a little at the wire and prussian blue appears there.

14110. Have rounded the angles and ends of three good crystals. Mounted them on wires. Waxed them once and then again as before (14065)—furnished the wires with flat loops, leaving the object wire and loop 5 inches long.

14111. Have constructed a small torsion balance in a glass jar so as to be sheltered from the wind—it can be raised and lowered by its stem or rod above. The suspender thread consists of 7 cocoon fibres, the bundle being 4 inches long. The beam is copper wire and the copper end is hammered thin and bent at right angles, the edges being up and down, so that a hook can hang over it in two directions. All this has been arranged at the great horse-shoe Electromagnet, where one pole piece terminates in a solid angle and the other presents a large distant surface. A finger jar to contain solution is placed against the pointed pole and in this the crystals are to hang in particular directions (14126).

14112. *Cobalt.* Repeated the observations, the arrangement being just as before (14092). The cube and cobalt in oil from first to last:

						× 2·2	currents
at 8h 48m—temp.		66°—right	134				
				278 − 118 =	160 at temp. of 66° F.	352	357·5[1]
		left	144				
				278 "	160 " " 66°	352	357·5
		right	134				
				278 "	160 " " 66°	352	357·5
		left	144				
9.0		spirit lamp	under the oil bath				
9.17	temp.	180°—right	136				
				286 "	168 " " 185°	370	375·5
9.19	"	190°—left	150				
				288 "	170 " " 194°	374	379·7
9.21	"	199°—right	138				
				288 "	170 " " 203°	374	379·7
9.22	"	207°—left	150				
9.31	"	242°—right	142				
				296 "	178 " " 245°	391·6	398
9.32	"	248°—left	154				
				299 [*sic*]	181 " " 250°	398	404·2
9.34	"	252°—right	143				
				296 "	178 " " 254°	391·6	398
9.35	"	256°—left	153	. . . .	. . . .	. lamp away	
				297 "	179 " " 250°	394	400
9.42	"	. . . . .		lamp on again			
9.42	"	243°—right	144				
				298 "	180 " " 241°	396	402·4
9.43	"	239°—left	154				

[1] This column is in pencil in the MS.

[at	9.43	temp.	239°—left	154]			× 2·2	
					297 − 118 =	179 at temp. of 240° F.	394	400[1]
	9.44	,,	240°—right	143				
					297 ,,	179 ,, ,, 242°	394	400
	9.45	,,	245°—left	154				
	9.55	,,	281°—right	146				
					302 ,,	184 ,, ,, 282°	405	411
	9.60	,,	283°—left	156				
					300 ,,	182 ,, ,, 284°	400	406·5
	10.1	,,	285°—right	144				
					299 ,,	181 ,, ,, 286°	398	404·2
					. . let a	little oil out because of the expansion		
	10.2	,,	287°—left	155				
					301 ,,	183 ,, ,, 288°	402·6	408·6
	10.4	,,	290°—right	146				
					300 ,,	182 ,, ,, 291°	400	406·5
	10.5	,,	292°—left	154				
					300 ,,	182 ,, ,, 293°	400	406·5
	10.6	,,	294°—right	146				
					300 ,,	182 ,, ,, 295°	400	406·5
	10.7	,,	297°—left	154				
					299 ,,	181 ,, ,, 297°	398	404·2
	10.8	,,	298°—right	145				
					300 ,,	182 ,, ,, 296°	400	406·5
	10.9	,,	294°—left	155				
					302 ,,	184 ,, ,, 292°	405	411
					. . . . .	. . . . . lamp flame diminished		
	10.12	,,	290°—right	147				
					303 ,,	185 ,, ,, 289°	407	413·3
					glass covers and shades on and then less motion as of currents—are in air in part at high temp.? When slow fall in temp.—then less of currents			
	10.13	,,	287°—left	156				
					304 ,,	186 ,, ,, 285°	409	415·8
	10.14	,,	283°—right	148				
					302 ,,	184 ,, ,, 283°	405	411
	10.15	,,	282°—left	154				
					302 ,,	184 ,, ,, 281°	405	411
	10.17	,,	280°—right	148				
					303 ,,	185 ,, ,, 279°	407	413·3
	10.18	,,	278°—left	155				
					301 ,,	183 ,, ,, 277°	402·6	408·6
	10.19	,,	275°—right	146				
					301 ,,	183 ,, ,, 275°	402·6	408·6
	10.21	,,	274°—left	155				
					303 ,,	185 ,, ,, 271°	407	413·3
	10.27	,,	267°—right	148				
					304 ,,	186 ,, ,, 267°	409	415·8
	10.28	,,	266°—left	156				
					305 ,,	187 ,, ,, 266°	411·4	417·9
	10.29	,,	265°—right	149				
					302 ,,	184 ,, ,, 264°	405	411
	10.32	,,	263°—left	153				
					302 ,,	184 ,, ,, 263°	405	411
					. . . . .	. . . . . flame reduced		
	10.33	,,	262°—right	149				
	10.45	,,	237°—left	148				

[1] This column is in pencil in the MS.

14113. *Cobalt* at different temperatures.

							× 2·2	
at	10.45	temp.	237°—left	148				
					298 − 118 =	180 at temp. of 236° F.	396	402·2[1]
	10.46	„	234°—right	150				
					302 „	184 „ „ 233°	405	411
	10.47	„	231°—left	152				
					299 „	181 „ „ 230°	398	404·2
	10.49	„	228°—right	147				
					299 „	181 „ „ 227°	398	404·2
	10.50	„	226°—left	152				
					299 „	181 „ „ 224°	398	404·2
	10.51	„	222°—right	147				
					297 „	179 „ „ 221°	394	400
	10.52	„	220°—left	150				
	11.0	„	207°—right	147				
					297 „	179 „ „ 206°	394	400
	11.1	„	206°—left	150				
					297 „	179 „ „ 205°	394	400
	11.2	„	204°—right	147				
					295 „	177 „ „ 203°	389·4	395·7
	11.3	„	202°—left	148				
	11.20	„	184°—right	146				
					296 „	178 „ „ 183°	391·6	397·9
	11.22	„	182°—left	150				
					295 „	177 „ „ 182°	389·4	395·7
	11.23	„	181°—right	145				
					296 „	178 „ „ 181°	391·6	397·9
	11.24	„	180°—left	151				
	11.50	„	168°—right	146				
					295 „	177 „ „ 168°	389·4	395·7
			left	149				
					295 „	177 „ „ 167°	389·4	395·7
			right	146				
					296 „	178 „ „ 167°	391·6	397·9
			left	150				
					295 „	177 „ „ 167°	389·4	395·7
			right	145				
					295 „	177 „ „ 166°	389·4	395·7
			left	150				
	11.56	„	166° . .	. . . .		. . . .	lamp away	
	12.49	„	103°—right	144				
					292 „	174 „ „ 103°	383	388·8
			left	148				
					292 „	174 „ „ 103°	383	388·8
			right	144				
					292 „	174 „ „ 102°	383	388·8
			102°—left	148				
	2.0	„	79°—right	143				
					289 „	171 „ „ 79°	376	382
			left	146				
					290 „	172 „ „ 79°	378	384·2
			right	144				
					292 „	174 „ „ 79°	383	388·8
			79°—left	148				

[1] This column is in pencil in the MS.

14114. As these powers at 79° are higher than those we started with (14112), so began to think that the metal either retains or acquires a charge slowly during the time of a long set of observations, and that this will affect the results at the beginning and end, and of course in some degree the order of the series. So I now turned the cube 180°, i.e. I reversed the cobalt in the magnetic field, left it so for five minutes, and then restored it to its former position and again observed, with the following results (14124).

14115. *Cobalt* at different temperatures—

						× 2·2	
at	$2^h\ 15^m$—temp.	78°—right	135				
				279 − 118 =	161 at temp. of 78° F.	354	360[1]
		left	144				
				281 ,,	163 ,, ,, 78°	358·3	364·8
		right	137				
				280 ,,	162 ,, ,, 78°	356	362
		left	143				
				279 ,,	161 ,, ,, 78°	354	360
		right	136				
				279 ,,	161 ,, ,, 78°	354	360
		78°—left	143				

14116. This is a curious change from the last observations, and shews the retention of charge. Now left it as it is to see if the power would gather up again.

at	$2^h\ 48^m$ temp.	75°—right	137				
				280 ,,	162 ,, ,, 75°	356	362[1]
		left	143				
				280 ,,	162 ,, ,, 75°	356	362
		right	137				
				280 ,,	162 ,, ,, 75°	356	362
		left	143				

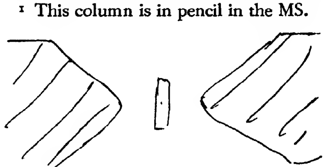
14117. So the force communicated at the reversion (14114) is pretty permanent. Probably high temperature aids much in helping either to lower opposing force or to gain like or accordant force (14124). Took the cube and cobalt off the balance and away.

14118. Made a torsion suspender of 10 fibres of cocoon silk and 10 inches long, continued it downwards by fine suspension copper wire and a flat loop—then took away the platinum torsion wire and put this in its place; so that the silk was in the upper part of the sheltering tubes and away from the action of heat, moisture or air currents: the loop at the bottom being then in its proper place.

14119*. The pole pieces of the Logeman magnet were now arranged with their pointed ends towards each other and 0·9 of an inch

[1] This column is in pencil in the MS.

* [14119]



14120. apart—air occupying the magnetic field. The *piece of granular bismuth* (13985) was placed on the torsion suspender and then the torsion between the upsetting points observed; it amounted to 10r 270°, or 3870°. So there is plenty of power here. The upsetting points were very wide apart ( ), for of course the place of greatest repulsion is near to the pole points, for there the power is strongest and its diminution most rapid.

14121. *Phosphorus.* The piece of phosphorus (13986) was then placed between the same poles at the same distance. The torsion force from point to point was about 840°. The vibrations are very slow and I am afraid currents may exert much influence here.

14122. *Heavy glass.* The piece of heavy glass (13984) placed between the same poles at the same distance: the torsion force from point to point was only 415°. The upsetting points 140° apart (or about) in air. It is difficult to bring the body up to them—for a little momentum soon causes it to pass over because of the want of power. One might make the torsion more sensible by removing some of the fibres, but that would tend to make the disturbing force of currents only the more sensible.

14123. The *Calcareous spar sphere* (13974) hung up between these poles at the distance of 0·9 of inch. The torsion force between the upsetting points was about 450°. The body was very slow in settling in a place, the time of vibration being very long. The sphere is heavy.

14124. *Cobalt.* Though it appears that a set is taken up (14114, 6) which the heat partly removes or affects, still it would appear that Cobalt has a higher force at the high temperatures, for the fall in the descending series with fall of temperature is very distinct (14112, 3). As if high temperature loosened its condition and allowed it to take a higher degree of charge—for it must be remembered that as the Cobalt prism is carried each time into a position in which its length is transverse nearly to the magnetic axis, so its state of charge in the magnetic field has to be continually reacquired.

14125. *Nickel.* The same may be said of nickel, as appears by (14097) and the curve drawn up from it.

14126. Red ferro pruss. potassa crystals (14110) and the little torsion balance in the jar (14111). Worked with them and a saturated solution of proto sulphate of iron now over crystals at the temperature of 65° F. This solution had a little sulphuric acid added to it, to keep it clear when diluted, and the addition of water or the saturated solution was made by volumes.

14127. In the Saturated solution, the crystals are well repelled, whether the axis of the crystal considered as a prism is equatorial or axial. In a medium consisting of 4 Vols. Solution and 12 vols. water, the crystal is attracted in either position.

14128. In a medium of 10 Solution + 6 Water, the crystal was attracted well with the side to the pole but attracted very little with the end on or toward it. With

Solution	Water		end to pole	side to pole
11 vols. +	6 vols.	–crystal–	repelled weakly	–attracted well
12 „ +	6 „	„	repelled fairly	–attracted well
13 „ +	6 „	„	„ „	attracted well
14 „ +	6 „	„	„ „	attracted well
16 „ +	6 „	„	„ „	attracted well
18 „ +	6 „	„	repelled strongly–	repelled feebly

14129. The second and the third crystals hung in this last medium gave exactly the same result.

14130. So with 10 vols. solution and 6 vols. of water the crystal thus coated with wax is always attracted. With 18 vols. solution and 6 of water–the same crystal and wax is always repelled. In a medium solution the crystal is attracted or repelled according to its position. A medium made of 2 vols. of solution and 1 of water answered exceedingly well. All the crystals were used. When the sides were towards the magnetic pole they were attracted, and when the ends were towards it they were repelled.

14131. One would like to find such a crystal in relation to space. Is it possible that a mixed carb. of lime and iron might do? I do not see any reason why it should not exist.

14132. Varied the Magnet terminations. Took away the large pieces of iron from the distant pole–the effect seemed just as good. Varied also the near or acting pole. When it was a blunt point it was best, but a vertical edge or a flat face $1\frac{1}{2}$ inches square did very well, so that there is no difficulty in the arrangement.

14133. Look at and consider in reference to this Magnecrystallic action the following references in the Exp. Res.

2552, 6, 9, 60, 74, 91.
2813–17, 28, 37, 9, 40, 1, 2.
2743, 4, 5—non expansion of gases.

14134. Granular bismuth bar (13985, 14120) at different temperatures—used the hot oil bath (14025, 43) with a copper cylinder in it for the object, but began by putting the oil into the bath, then the copper cylinder *with water*, then placed the whole in a jug with ice round it to bring all to 32°. Stirred up the thickened oil with a proper stirrer—put a piece of ice in the water of the cylinder and when all was well cooled, quickly put on the flannel jacket and put the bath, etc. between the pointed poles at the magnet (14120); the poles were brought close up to the bath and the points were 1·2 inches apart. The bismuth bar was cooled in ice, then put into its place on the balance and an observation made as quickly as might be. I did not note the zero of graduation, but bringing the bar to its upsetting point on one side, I then noted the number of degrees between it and the other side. The *torsion suspender is of silk* (14118).

14135.

Temp.	40°—from right to left point	3^r 285° = 1365 − 108 =	1257 at 40°
	44° ,, left to right ,,	3^r 302 = 1382 ,, =	1274 ,, 44°
	48° ,, right to left ,,	3·297 = 1377 ,, =	1269 ,, 48°

14136. Then let the temperature rise, leaving all the adjustments unchanged—at 2^h 19^m put a spirit lamp under the bath—also moved the object cylinder towards one end and stirred up the oil, and after a time took the lamp away—and then observed.

Temp.	79°—from right to left	1325 − 108 =	1217 at 79°
	76° ,, left to right	1343 ,, =	1235 ,, 76°
	,, right to left	1348 ,, =	1240 ,, 75°
	,, left to right	1350 ,, =	1242 ,, 74°
	73° ,, right to left	1350 ,, =	1242 ,, 73°

14137. So far the observations are good—and also accordant with crystals.

14138. The upsetting angle of the *bismuth bar* was 108°.

14139. Took off the bismuth—took away the cylinder and water—

put a cylinder and oil in its place—replaced the bar and proceeded to raise the temperature of the bath by the spirit lamp.

					. . . lamp away
At	$3^h\ 21^m$—temp.	128°—right	1220		. . . motion of object very slow
				1875	
	3.30 ,,	114°—left	655		
				1835	
	3.40 ,,	105°—right	1180		. . . very slow motion
	3.50 ,,	100° . .	strange apparent rise in power, and strange retardation—put the lamp under again.		
	4.8 ,,	245°—right	1340		
				2924	
	4.17 ,,	300°—left	1584		
				2674	. currents
	4.24 ,,	280°—right	1090		
				2530	
	4.29 ,,	280°—left	1440		
				2675	
	4.37 ,,	288°—right	1235		. . something very troublesome
				1955	
	4.43 ,,	289°—left	720		—then on to 1080—soon up to 1180 the temp. now 290°.

14140. Seems as if the silk quickly set, which is not likely. Must be something else. Appears to be caused by the change from water to oil as the medium—but oil is less diamagnetic than water—so cannot be in that relation. May be electric, as hot oil and metal is concerned, or may be something else.

14141. Let the temperature fall—removing the lamp altogether, then

at	$5^h\ 2^m$—temp.	220°—right	315		
				1855	
	5.7 ,,	200°—left	1540		. . . . very unsteady now
	,,	184°			

14142. Now took out the bismuth, and found some cotton or woollen filaments adhering to it at the copper wire holder, and have no doubt these have caused my trouble. I have used oil in the cylinder that has been in the bath, and do not wonder at filaments in it. New oil must be used. The filaments may have been carried to the metal by the currents that must occur or may have adhered because of Electric excitement. I think they must have rubbed against the sides of the copper cylinder, and so

causing obstruction, have required the extra torsion force shewn by the index.

14143. The observations above (14139) are made from a zero which, though not accordant with the point of no torsion, was still a fixed point during all the observations, and therefore gives the amount of torsion from one side to the other.

14144. Assuming an average force and temperature (14135), it would give about 1266 torsion force at 44° F., and doing the same for (14136) it would give an average torsion force of 1235 at 75°·4 F. The difference of 31 torsion force is a diminution in power of $\frac{1}{40}$ of the force at 44°. Taking the forces for the same temperatures from the curve of (14036), they are 153 at 44° and 141 for 75°·4—the difference 12, which is in the same direction as regards temperature, is $\frac{1}{13}$th of the power at 44° F. This is a great difference, but the comparitive points of observation are too few as yet.

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14145. Magnecrystal in intermediate media. Wish to find one that includes a vacuum between its differences, and tried calcareous spar, using the apparatus before described (14111, 26) and the sphere of Calc spar given to me by W. Thomson (13593)—on first in water, for as Air is paramagnetic and water diamagnetic, the calc spar, which is diamagnetic to the former, may still be paramagnetic to the latter and so come near to or on space.

The optic axis axial—the sphere attracted very slowly
" " equatorial— " attracted more than before.

Calc. spar therefore is less diamagnetic than water and so nearer to vacuo.

14146. Carbonic acid gas is at Zero, i.e. is like a vacuum, therefore tried the sphere in that gas.

The optic axis axial—the sphere repelled well.
" " equatorial— " repelled well but not quite so much.

14147. So the Carb. lime is diamagnetic in relation to Carbonic acid and to space, and that in both positions. That the attraction in water is most when the Optic axis is equatorial and the repulsion least in Carbonic acid for the like position is a consequence of the

constancy of the Magnecrystallic difference, and accords with the belief that it would be so with mere space as a medium, i.e. that the full magnecrystallic difference would exist.

14148. As it is also attracted in water, so the addition of a little of the salts of iron, nickel or cobalt would give a like fluid medium. Or diluted Alcohol would give a like medium, for

14149. In Absolute Alcohol, the Calc. Spar sphere
Optic axis axial—the sphere is repelled
" " equatorial— " is repelled but not so much as when the O. axis is axial, and indeed only very slowly—but the mass of fluid obstructs the motion as compared with carbonic acid gas.

14150. Perhaps some of the crystals of Calc. spar which contain a little iron might probably be found to coincide with space. I have a small rhomboid which Tyndall gave me; its short or optic axis points axially, therefore as a paramagnetic crystal. In Air the crystal was attracted whether the optic axis was axial or equatorial, and of course it would do so in Carbonic acid gas, or in space. On trying it in Carbonic acid such was the case, the attraction being far stronger than in air, as it ought to be.

14151. It is evident that an intermediate crystal between this and that of pure Calcareous spar may be found. Such crystals allow of adjustment of their Magnecrystallic zero, i.e. of their magnetic relation, just as media do.

14152. *Granular bismuth bar*, different temperatures. The oil bath, magnet and magnet pieces were left on the last occasion undisturbed; the distance of the pole points is 1·2 inches (14134). The torsion fibre is of several silks (14118). The bismuth bar was carefully examined and found in good condition. To give some degree of freedom of motion to it at low temperatures, Camphine was put into the cylinder instead of oil—it is very near in diamagnetic relation to it. Examined for the *upsetting angle* and made it out to be 114°. The temperature is 65° F.

14153. Revolved the torsion index to the Right to the upsetting point—from that proceeded to the left upsetting point—it required 5^r 308 = 2108 degrees of torsion! (14135, 6). Then placed the torsion index at Zero and turned right—2^r 226° = 946 } 1934 from
left 2^r 268° = 988 }
point to point, the temperature being 65° F.

14154. This great difference between 1250 (14135, 6) and 1934 at temperatures nearly the same must be considered. It may have depended upon the fibres of the silk bundle having settled into place in respect of each other—but renders the quality of the suspension doubtful.

At 2^h 10^m put the spirit lamp under the oil bath, leaving the bismuth in the camphine—at 2^h 17^m the lamp removed—

2^h 18^m—temp. 135°—right 4^r 70° = 1510 of torsion ⟩ 3050
2.24^m „ 130°—left 4^r 100 = 1540 „

14155. This is an enormous increase on the former (14153); moreover it seems by the effects as if the torsion suspender took set or gave way very easily. I suspect the vapour of the camphine effects it greatly. Returned the index to 0°. Then proceeded to the right and found it required 3^r 180°, but in a few minutes it required 90° more and if I had waited would I think have required much more. The temperature was 117° F. and there were evidences of currents in the fluid.

14156. Removed and examined the bismuth: could see nothing wrong about it; boiled it in Alcohol and cleaned it thoroughly. Took away the camphine and cylinder from the oil bath. Removed the tube from around the torsion thread to air it and displace the vapour of camphine.

14157. Put another cylinder with pure filtered olive oil into the trough and the bismuth bar into it. The power of the magnet is so little and the oil so thick that the motion of the bismuth is exceedingly slow—is as if it were in mud. Being placed at an angle of 45° with the magnetic axis, it went very slowly into its place, i.e. into the equatorial position. Put up only a half side of the torsion protecting tube, so as to shelter it and yet to allow the air and the removal of camphine vapour.

14158. Adjusted the bismuth prism equatorial and the torsion index at Zero: then 4^r not enough to upset it (they were to the left); one revolution more, i.e. 1800°, after a good while, slowly upset it. Returned through 5^r to Zero: the prism moved very slowly towards zero. Added 5 right revolutions—still moved slowly but at last passed the upsetting point. Returned the index to Zero—the bismuth returned very slowly. Will not do.

14159. At 3^h 10^m put the lamp under for an observation or two

when the oil was hot. At 200° the bismuth moves more freely, but still very slowly. At 250° moves better. At 292°, still better, but currents now tell. Right torsion 2^r 200 = 920° upset the bar, and again 3^r = 1080 left torsion upset it, making 2000° torsion force from point to point. Temp. 278°—but the observations very vague and uncertain. Set of torsion fibre—currents, etc. etc. No use going on this way. Must use a wire.

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14160. Replaced the fine silver torsion wire in the balance (13736), dismissing the silk bundle (14118).

14161. Made some observations as to the sensibility when different objects were in the magnetic field, but forgot at first to move the cross piece of soft iron from the arms of the magnet, so that its power was much diminished.

14162. The *Carb. iron crystal* with its optic axis vertical (13976), the pole points being only 1·25 inches apart. The torsion force with this silver wire was only 380° from one upsetting point to the other.

14163. *Tourmaline*—a small dark blue or black crystal, 0·62 of an inch long and 0·1 of an inch in diameter. Was magnetic but shewed no sensible power with this wire.

14164. The *granular bismuth bar* (13985). Scarcely sensible. The bar easily stood axially.

14165. *Carbonate of Iron.* The crystal (13976), in order to make it more sensible paramagnetically, was ground down into a flat plate, the directions of the optic axis and of the length being left untouched. When suspended with the optic axis vertically, its height was 0·37 of an inch—its length 0·6 of an inch—and its breadth 0·17 of an inch.

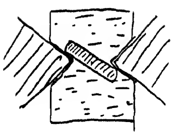
14166. The cross piece of soft iron (14161) was now discovered and taken away and the objects tried again.

14167. *Carb. iron plate* (14165) with optic axis vertical and the pole points 2·3 inches apart—torsion force from point to point was 190°. With a magnetic field 2 inches across, the force was 235° about. With an interval of 1·8 inches, the force was 300°. At this distance the carb. of iron tends to go to the poles, being attracted by them, and if in oil, being buoyed up, it would probably

go to them. This will not do: it must not leave its place. I must hang a little copper cube (13987, 98) on to it to make it heavy.

14168. The *tourmaline* (14163) with a copper cube (13987, 98) hanging to it, in the magnetic field, the interval being 1·8 inches. No sensible effect with this torsion wire. Made the interval 1·25 inches, or trough width–still the same insensibility. In certain positions corresponding to the upsetting points the tourmaline is pulled round a little towards the axial position, but so slowly that I cannot distinguish them. In fact it goes round the circle gradually as the torsion wire goes round, but not equably in every part; the space passed through by it is more or less for a like number of torsion degrees, but produces no distinct upsetting points.

14169. The bismuth prism (14152) with the poles close up to the bath. The torsion force from point to point 184° about, not more; and when the bar swung past the second upsetting point it did not move on above 30° or 35° more, so as to be nearly equatorial though on the other side of the magnetic axis. This is not power enough. Perhaps a trough and poles of this kind might do, bringing the bismuth back by torsion to a constant position near to the pole points.



14170. Proceeded to work with the *Carbonate of Iron plate* (14165) for effect of temperature on it as a mere paramagnetic body. One of the small copper cubes (13987, 98) weighing 44 grains was hung on beneath it. The oil bath had thin pieces of wood between it and the pole points, so that they were 1·95 inches apart. The trough with its oil and a cylinder containing camphine (to give a free motion at low temperatures) and the crystal and cube was cooled in ice as before (14134)–then the flannel jacket put on and the whole put quickly into its place at the magnet–then

temp.								
46° F.	whole torsion force	from the	left	to the	right	upsetting	point	= 287°
46°	„	„	right	„	left	„	„	= 280°
46°	„	„	left	„	right	„	„	= 280°

14171. The *upsetting angle is* 108° ascertained by observation } –the above forces therefore are { 170°, 172°, 172° } at 46° F.

14172. Had forgotten to attach the governing bristle to the hook

of the object (   ) but I found that a piece of straight fine copper wire or a bristle, dropped into and resting in the angle at the junction of object and torsion wire, made an excellent indicator, etc., better than the attached bristle.

14173. Placed the torsion index at Zero, when the carb. of iron was in the axial position. Placed the spirit lamp under the trough a little while and then took it away.

temp.	89°–right	132			F.		
			263 – 108 =	155 at temp.	88°	97[1]	96·8[1]
,,	88°–left	131					
			262 ,, =	154 ,,	87°	95·8	95·7
,,	86°–right	131					
			262 ,, =	154 ,,	86°	94·7	94·6
,,	86°–left	131					

lamp under again a while—then away

,,	126°–right	124					
			251 ,, =	143 ,,	125°	137·5	currents
,,	123°–left	127					
			254 ,, =	146 ,,	122°	134·2	
,,	120°–right	127					
			253 ,, =	145 ,,	120°	132	
,,	119°–left	126					

lamp under again—then after a while reduced the flame

,,	146°–right	125					
			249 ,, =	141 ,,	147°	161·7	currents
,,	148°–left	124					
			250 ,, =	142 ,,	149°	163·9	
,,	151°–right	126					
			248 ,, =	140 ,,	152°	167·2	
,,	153°–left	122					

Carb. iron, long bar, at different temperatures, as a paramagnetic body.

14174. Removed the camphine and its cylinder, replacing it by another cylinder and pure olive oil, restoring the object to its place, successfully. Observed after a time, when all had gained a common temperature, no lamp being beneath.

temp.	136°–right	125		. . . moves very slowly—but quick enough—object goes steadily to the upsetting angle and then passes off.
			251 – 108 =	143 (412)[2] at temp. 134° 157·3
,,	132°–left	126		
			251 ,, =	143 (412) ,, 130° 157·3
,,	127°–right	125		
			251 ,, =	143 (412) ,, 126° 157·3
,,	125°–left	126		
,,	115°–left	132		. . . . lamp under and left

[1] This column is in pencil in the MS.
[2] Figures in brackets are in pencil in the MS.

Carb. iron bar at different temperatures.

at	1^h 30^m temp.	200°—right	122					× 1·1	
				244 − 108 =	136 (394)[1]	at temp.	208°	149·6	
	1.32 „	216°—left	122						
				245 „ =	137 (396·2)	„	221°	150·7	
	1.33 „	227°—right	123						
				242 „ =	134 (388)	„	230°	147·4	currents
	1.35 „	234°—left	119						
				236 „ =	128 (370)	„	239°	140·8	
	1.37 „	244°—right	117						
				234 „ =	126 (364·2)	„	248°	138·6	
	1.39 „	253°—left	117						
				235 „ =	127 (367·2)	„	256°	139·7	
	1.41 „	259°—right	118						
				233 „ =	125 (362)	„	264°	137·5	
	1.43 „	269°—left	115						
				232 „ =	124 (358·8)	„	272°	136·4	
	1.45 „	276°—right	117						
				231 „ =	123 (356)	„	279°	135·3	
	1.46 „	283°—left	114						
				231 „ =	123 (356)	„	286°	135·3	
	1.48 „	289°—right	117	. . . . .	. . . . . .	. . .	. .	smaller flame	
				229 „ =	121 (350)	„	291°	133·1	
	1.50 „	294°—left	112						
				222 „ =	114 (330)	„	292°	125·4	
	1.52 „	289°—right	110						currents
				223 „ =	115 (332·1)	„	288°	126·5	
	1.53 „	287°—left	113						
				223 „ =	115 (332·2)	„	287°	126·5	
	1.55 „	286°—right	110						
				223 „ =	115 (332·2)	„	286°	126·5	
	1.56 „	285°—left	113						
				224 „ =	116 (335·5)	„	285°	127·6	
	1.57 „	285°—right	111						
				223 „ =	115 (332·2)	„	285°	126·5	
	1.59 „	284°—left	112						
				222 „ =	114 (330)	„	284°	125·4	
	2.0 „	284°—right	110	. . . . .	. . . . . .	. . .	. .	lamp away	
	2.9 „	258°—left	111	. . . . .	. . . . . .	. .		lamp under, small	
				227 „ =	119 (344)	„	255°	130·9	
	2.11 „	252°—right	116						
				229 „ =	121 (350)	„	250°	133·1	
	2.13 „	247°—left	113						
				232 „ =	124 (358·8)	„	247°	136·4	
	2.14 „	246°—right	119	. . . . .	. . . . . .	. . .	. .	lamp away	
	2.24 „	220°—left	117						
				241 „ =	133 (384·8)	„	218°	146·3	lamp on, small
	2.26 „	215°—right	124						
				241 „ =	133 (384·8)	„	214°	146·3	
	2.28 „	213°—left	117						
				241 „ =	133 (384·8)	„	213°	146.3	lamp away
	2.29 „	214°—right	124						

[1] Figures in brackets are in pencil in the MS.

Carb. iron bar at different temperatures.

at	2.46	temp.	178°–left	118					× 1·1
					250 – 108 =	142 (410·8)[1]	at temp.	176°	156·2 lamp on, small
	2.47	„	174°–right	132					
					252 „ =	144 (416·5)	„	174°	158·4
	2.49	„	173°–left	120					
					252 „ =	144 (416·5)	„	174°	158·4
	2.51	„	175°–right	132	. . . .	. . . . . .	lamp away altogether		
	3.8	„	146°–left	122					
					254 „ =	146 (422)	„	145°	160·6
	3.10	„	144°–right	132					
					254 „ =	146 (422)	„	143°	160·6
	3.11	„	141°–left	122					
					254 „ =	146 (422)	„	140°	160·6
	3.13	„	139°–right	132					
	3.54	„	100°–left	131					
					274 „ =	166 (480)	„	99°	182·6
	3.57	„	98°–right	143					
					274 [*sic*] „ =	166 (480)	„	97°	182·6
	4.1	„	96°–left	133					
					274 [*sic*] „ =	166 (480)	„	96°	182·6
	4.5	„	95°–right	144					

14175. So there is a loss of power by elevation of temperature and that accords with the crystal. But the loss here seems to be less than with the crystal, for here at 96° the force is 166 and at 292° the force is 114, being a loss of $\frac{1}{3\cdot 2}$ of the force at 96°. Whereas with the crystal the force at 96° is 457 and at 292° it is reduced to 246, the loss being as much as $\frac{1}{2\cdot 11}$ of the force at 96°. If however the medium also loses force, being here diamagnetic substances, then the loss of the prism might be affected, whereas such a change has no influence over the Magnecrystal.

14176. If the medium lost in diamagnetic force, then the paramagnetic character of the carbonate of iron ought to fall faster than if it remained unchanged, and that loss would be added on to the one above, which would make it still more different from the crystal. Must consider these points and their consequences.

14177. *Fibrous iron Magnecrystallic?* Have had some small cylinders turned out of fibrous iron, of smaller and larger sizes, so that the fibre is across the cylinders and is parallel to the plane of the little copper hooks, by which they are to be suspended. Hung one up by a single cocoon silk fibre and then approached

[1] Figures in brackets are in pencil in the MS.

a horse shoe magnet horizontally towards it, to observe whether it would set with the fibres parallel to the lines of force. It did vibrate about a given position, but this was indifferent, for when the cylinder was held for a second in any position and then left to itself, it then set in and vibrated about that position, having taken a magnetic charge. One of the cylinders was carefully ignited, cooled and then subjected to the distant action of the horse shoe magnet (a small one), but the direction of the fibres was perfectly indifferent and there was no sign of magnecrystallic action. The cylinders required great care in their suspension that they should be perfectly vertical.

14178. As to media, cannot tell at present whether even a medium at 0° made up of water and Sul. Iron would change or not:—or even whether space would change its relation. Most interesting questions arise here.

14179. Carbonate of Iron bar (14173, 4): in the curve[1] or line laid down there is the same apparent straight line as in the crystals of bismuth (13925): has no reference to bismuth.

14180. Very cold bismuth might make a powerful temporary magnet. What would it be as a Magnecrystal?

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14181. Red ferro pruss. potassa, heated in oil, at last blackens, then breaks up suddenly with a sort of explosion—being afterwards washed by camphine and water, it gave a dark brown solution and left Prussian blue. Heated in a tube in air, it changed and broke up in like manner, evolving prussic fumes. It is the heat which changes it.

14182. The great horse shoe magnet arranged with 10 pr. Grove's plates and pole pieces with opposed flat faces 3·4 × 2 inches and 1·1 inches apart. A crystal of red ferro pruss. pot., held by copper wire and loop from a single cocoon thread, pointed well between the poles—almost as strong as tourmaline. Then being gradually heated by a spirit lamp, it pointed well Magnecrystallically, though with diminishing power; until it broke up and was dissipated it never lost its Magnecrystallic character.

14183. The *tourmaline* crystal (13691)—pointed well between

[1] See note following par. 14187.

these poles when 1·5 inches apart, i.e. powerfully. Being gradually heated by a spirit lamp, it was raised to a red heat, but never lost its magnecrystallic character, though it diminished in power. As it cooled it regained its power. When cold it was digested in Mur. acid to remove any oxide of iron that the heat and Alcohol vapour might have changed (   ).

14184. *Proto carbonate of iron*—a crystal of this substance pointed with optic axis axially when poles far apart—its powers are very great. A crystal, hung by copper wire and heated by spirit lamp, decrepitated suddenly and powerfully. By heating a crystal first at the sand bath and then by the lamp, it cracked, but held together and seemed changed, being black, etc.: but lost it; must try again.

14185. Tyndall's rhomboid of calc. spar, of which the optic axis points axially, was broken up into smaller rhomboids each of which pointed in like manner. On dissolving some of it in pure M.N.Acid, etc., I found a trace of iron in the crystal, but not much.

14186. Examined some Calcareous spar for further specimens of like Magnecrystallic character and found three pieces. All of these gave traces of iron as the former did. Two of them had faint veins here and there, of a dark green colour, like particles of chlorite or something proto ferruginous (pyrites).

14187. A piece of this ferruginous Carb. lime heated flew to pieces. Must proceed carefully and slowly.

[The results for carbonate of iron bar (14173, 4) were plotted in graphs included in the MS. at this point. They have not been reproduced.]

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14188. *Pure Calc Spar* (   ). Rhomboid, suspended by *copper binding wire*, held above by a single cocoon thread—between the flat poles of the great Electro magnet (14182)—it pointed with short axis equatorially. Being gradually raised in temperature over a gas flame and then transferred to the magnetic field, it was gradually heated by a spirit lamp up to a red heat. During the whole time it remained Magnecrystallic, the short axis pointing equatorially—it never changed in character. A part of the exterior of the crystal beneath, where the heat and watery vapour acted most favourabl[y], was converted into caustic lime. Still, at the

highest temperature, the crystal pointed and as at common temperatures. As it cooled it continued to possess the same state. I though[t] the directive force might be less than before.

14189. The suspending copper wire is more magnetic than it ought to be. A bundle of it at common temperatures sets axially and is attracted when cold. When hot it is indifferent, or if anything equatorial in air, etc.

14190. Platinum wire, fine, a bundle or ring coil, was not so magnetic as the cold copper when itself cold, and when heated still pointed slightly axially. Better to use it than the copper wire.

14191. A crystal of *Carb. Prot oxide of iron*, slung with copper and hung at the top of a lamp glass with a low flame, bore a temp. up to 200° or about for 1½ hours very well, but then being made a little warmer, it exploded like a Rupert's drop. So also did two other crystals.

14192. A crystal of Carbonate of Iron, put with olive oil into a tube and heated—it bore the first temperatures very well, but at a certain high temperature it suddenly broke up as before. Some pieces were left amongst the smaller powdery part—the largest of these was selected, mounted on platinum wire and suspended in the magnetic field. It proved to be as Magnecrystallic as before. It was then gradually heated by a spirit lamp, and then gradually broke away, but a part remained for some time—this was magnecrystallic, as the cold crystal in kind but not in degree, for the power became less and less—at last the wire and remaining fragment was dull red hot, but still the part which remained was magnecrystallic, though weakly—at last that portion decrepitated and disappeared from the wire. The effect was very good as far as it went.

14193. Another Rhomboid of Carb. iron was slung in platina wire, being wrapped round in two directions, and then suspended in the Magnetic field. It was strongly Magnecrystallic. It was then put for a little while over the Argand lamp—returned to the magnetic field—heated by a spirit lamp applied beneath successfully and tried from time to time by exciting the magnet; it was also Magnecrystallic and in the same manner, but grew weaker in power as the temperature rose. At a dull red heat it was magnecrystallic. It then seemed to lose Carbonic acid rapidly by the

appearance and also all magnecrystallic power—it was a little magnetic at the high temperature, pointing with its length axially, but feebly; but on taking away the lamp and letting the temperature fall, it soon became exceedingly magnetic, loose pieces flew from it to the magnetic poles and it found no place of rest in the middle distance. It was in fact very magnetic oxide of iron, opake, and contained no carbonic acid.

14194. As long as the crystal remained a carbonate it was Magnecrystallic.

14195. Ferro carbonate of lime crystallized (14185, 6), i.e. such carbonate of lime as in the Magnetic field sets with the optic axis axially. Hung some pieces slung by copper wire over the gas lamp (14187). After some time, two flew to pieces, though the heat not enough to scorch paper. Put a thermometer in the place: temp. 220°. Hung up other crystals from other pieces; at 260° began to fly—at 300° the rest broke up like a Rupert's drop. With another crystal, about one half remained as a piece fractured throughout—hung it up in the Magnetic field—it then bore the heat of the spirit lamp pretty well and with the following result. When below a certain temperature it pointed with the optic axis axially, but on raising the temperature considerably, it pointed with the optic axis equatorially. On lowering the temperature, this was changed; and again when raised in temperature, the change was reversed. The crystal pointed Magnecrystallically at all times, but at the lower temperatures it pointed as a rhomboid of Carb. of iron and at the upper temperatures as a rhomboid of Carbonate of lime. When cold it was attracted as a whole, being paramagnetic.

14196. Another rhomboid of the Ferro carbonate of lime was put into a tube with oil and heated—at about 300°. Much of it burst into dust, but even after raising the oil to a decomposing temperature some fragments of the Rhomboid remained. One of these was selected and slung by the platina wire—hung in the magnetic field and tried: it set magnecrystallically, as Carb. Iron. When the spirit lamp was applied, the fragment remained after the oil was dissipated and burnt away, and now it always pointed Magnecrystallically; but when above a certain temperature it pointed as pure calc. spar, when below as carb. iron—confirming and enlarging the former result (14195).

14197. *Compressed carbonate of iron.* Tyndall gave me a rhomboid of Carbonate of iron powder pressed together in one direction—the rhomboid pointed with the short axis axially—being suspended in the magnetic field, a very short application of the spirit lamp took away this power, which did not return by cooling—and very soon parts were decomposed and the magnetic oxide of iron set free. Might make the experiment better in heated oil.

14198. The ferro carbonate of lime spoken of contains very little iron (14185) but some of its properties seem much affected, especially its cohesion. The manner in which it flies to pieces by a certain elevation of temperature, as if it were then really another body, seems to shew that there must be contending actions going on within side; and its change from the paramagnecrystallic to the diamagnecrystallic state accords with this view. Probably the optic qualities would alter in some manner and they seem deserving of examination. The substance is I think harder than the regular calcareous spar and always more fissured. As far as I have traced it, the fine veins of minute crystals of pyrites accompanies the presence or production of this kind of Calcareous spar.

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14199. A, a calcareous spar from Mr Tennant. A little rhomboid of it set between flat faced poles (   ) with the optic axis equatorially.

B, another calcareous spar from Mr Tennant—a rhomboid of it set with the optic axis equatorially.

C, another calcareous spar Do.—rhomboid sets with optic axis equatorially.

D, another Calcareous spar Do.—rhomboid sets with the optic axis equatorially.

E, another calcareous spar—labled from Wicklow in Ireland, very lustrous and having something of the topaz look about it—also with many conchoidal or cross fractures—a rhomboid of it sets with the optic axis equatorial, like the rest.

14200. Four crystals of Tantalite from Greenland (from Mr Tennant). All were strongly magnecrystallic and set with the length (when they had length) equatorially. Certain planes faced to the flat poles, and these were I think the planes M in the figure

in Thomson's Mineralogy[1], Vol. 1, p. 485; but the crystals I have do not well identify with that figure. The planes I speak of join on to other planes which surround the crystal, and the linear angles are parallel to each other and to an axis round which the crystal may be revolved—but this happens in two directions which are at right angles to each other, so that probably the planes are not difficult to recognise in other crystals.

[1] *Outlines of Mineralogy, Geology and Mineral Analysis.* By Thomas Thomson, M.D. London, 1836.